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Space as Form of Intuition and as Formal Intuition: On the Note to B160 in Kant's *Critique of Pure Reason*

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The footnote B160–61n that Kant appended to section 26 of the B-version of the Transcendental Deduction (TD) in the *Critique of Pure Reason* (CPR) addresses the relation of the unity of space to the faculty of understanding. This has been the source of much puzzlement among commentators, and the disagreements to which this has led have often had far-ranging consequences for one's approach to what is arguably

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Abbreviations of Kant's Works

AA = *Akademische Ausgabe* (Kant 1900–)
CPR = *Critique of Pure Reason* (Kant 1998)
DDS = *Concerning the Ultimate Ground of the Differentiation of Directions in Space,* in AA 2:375–84
Disc = *On a Discovery Whereby Any New Critique of Pure Reason Is to Be Made Superfluous by an Older One,* in AA 8:187–251
LF = *Thoughts on the True Estimation of Living Forces and Assessment of the Demonstrations that Leibniz and Other Scholars of Mechanics Have Made Use of in This Controversial Subject, together with Some Prefatory Considerations pertaining to the Force of Bodies in General,* in AA 1:1–181
Refl. = *Reflexionen,* in AA 16–19

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Kant's central argument in TD. Thus Martin Heidegger (1991, 146n203; 1995, 132ff.) supports his radical interpretation of Kant by explicit reference to this footnote, using it to shore up the central role he gives to the imagination in TD. Béatrice Longuenesse (1998a, 219) uses it as evidence for her controversial interpretation of TD and in particular of the understanding's effect upon sensibility. The recent debate around nonconceptual content in Kant (Wenzel 2005; Hanna 2005, 2008, 2011; Allais 2009; Grüne 2009, 2011; Griffith 2012; Schulting 2012a, forthcoming a; Tolley 2013) is directly related to this footnote insofar as it might be read as supporting a conceptual account of what the Transcendental Aesthetic (TAe) first presented as nonconceptual. And lastly, this issue has also proven to be relevant to the recent discussion around the role of constructions in geometry (Shabel 2003; Friedman 2012), because the epistemological significance of an apparent need to resort to diagrams in Euclidean geometric reasoning depends upon the extent to which space is independent of conceptual content.[1]

Although a considerable amount of literature has already been devoted to the footnote, there is however little consensus around its interpretation. This article seeks to address the problematic interpretative issues that the footnote raises, by reference to key readings in the literature. Following the identification of three central problems of interpretation in section 1, section 2 identifies two main interpretative strands (which we label the *Conceptualist* and the *Nonconceptualist* approaches) in terms of their understanding of one of these problems, and their response to the other two. The problems faced by these approaches are examined in more detail in section 3, once the notion of "unity of space" (B160n) has been clarified by reference to TAe. This leads to viewing a broadly nonconceptualist approach as the most promising interpretation: the principal idea at the core of our proposal is presented in section 4, and we show how it provides an account of Kant's distinction between "form of intuition" and "formal intuition" in the second part of the B-version of TD (sec. 26). This idea is then further developed to examine the related issues of the concept of space (section 5), the role of the categories in determining space through synthesis (section 6), and the role of the imagination in the synthesis of space (section 7). Finally, in section 8, the proposed interpretation is used to clarify our responses to the issues raised in the three problems identified at the outset.

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[1] If spatiality cannot be formalized and thereby represented in purely conceptual terms, then geometry, as the science of space, cannot proceed without spatial representations playing a central role in geometric proofs. There are, however, different views as to how diagrams function in geometric proofs, with a prominent Platonic view that one somehow accesses the truth by "seeing" it in a geometric representation (Brown 2008, 26–50). This is incompatible with Kant's view of the role of diagrams in geometry as involving constructions through which the objects of geometry are determined by means of synthesis. For Kant, a synthetic geometric judgment is not made by first drawing a diagram and then observing (in the mind's eye, if this diagram is drawn in thought) the truth of a certain geometric proposition. Rather, it is in drawing an intuitive representation of the diagram that the synthetic proposition is formed, because the act of drawing consists in the act of synthesizing a region of space according to the rules of Euclidean geometry. Through inferences guided by this construction, it is possible to establish the truth of a geometric proposition (A716–17/B744–45).

1. Interpretative Problems

The footnote, appended to B160, reads:[2]

> Space, represented as *object* (as is really required in geometry), contains more than the mere form of intuition, namely the *comprehension* of the manifold given in accordance with the form of sensibility in an *intuitive* representation, so that the *form of intuition* merely gives the manifold, but the *formal intuition* gives unity of the representation. In the Aesthetic, I ascribed this unity merely to sensibility, only in order to note that it precedes all concepts, though to be sure it presupposes a synthesis, which does not belong to the senses but through which all concepts of space or time first become possible. For since through it [that is, through synthesis] (in that the understanding determines the sensibility) space and time are first *given* as intuitions, the unity of this *a priori* intuition belongs to space and time, and not to the concept of the understanding. (§24; translation amended)[3]

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[2] All quotations from the *Critique of Pure Reason* (henceforth CPR) are from Kant 1998. We follow the standard way of reference to the A/B editions of CPR. All other works by Kant are referred to by reference to the respective volumes of Kant 1900– (abbreviated AA), followed by volume and page numbers.

[3] The text in the *Akademische Ausgabe* (AA 3:125n) reads:

> Der Raum, als *Gegenstand* vorgestellt (wie man es wirklich in der Geometrie bedarf), enthält mehr als bloße Form der Anschauung, nämlich *Zusammenfassung* des Mannigfaltigen nach der Form der Sinnlichkeit Gegebenen in eine *anschauliche* Vorstellung, so daß die *Form der Anschauung* bloß Mannigfaltiges, die *formale Anschauung* aber Einheit der Vorstellung giebt. Diese Einheit hatte ich in der Ästhetik bloß zur Sinnlichkeit gezählt, um nur zu bemerken, daß sie vor allem Begriffe vorhergehe, ob sie zwar eine Synthesis, die nicht den Sinnen angehört, durch welche aber alle Begriffe von Raum und Zeit zuerst möglich werden, voraussetzt. Denn da durch sie (indem der Verstand die Sinnlichkeit

Lorne Falkenstein describes this footnote as "so obscure that it can be made to serve the needs of any interpretation whatsoever." He continues, "From a contradiction anything follows, and any text that contains two assertions like (a) 'this unity [of space and time] . . . presupposes a synthesis . . . through which all concepts of space and time are first made possible', and (b) 'the unity of this intuition [of space and time] belongs a priori to space and time and not to the intellectual concept' is close enough to exhibiting a contradiction that it makes it possible to get virtually any conclusion one pleases out of the passage with only minor effort" (Falkenstein 1995, 91). Three issues can be identified that are potentially the source of interpretative disagreements:

1. The apparently contradictory nature of the second sentence of the footnote: while it is claimed that the unity of space "precedes all concepts," it also "presupposes a synthesis," and moreover, this synthesis makes "concepts of space and time first become possible." There is a prima facie tension here, for, according to Kant, every synthetic activity involves the understanding (B130), that is, the faculty which unites manifolds under concepts. So if the unity of space precedes all concepts, then it should surely not presuppose a synthesis.[4]
2. At the outset of the footnote, the statement that space is represented as an object could be interpreted as implying that it is indeed to be seen as an object. However, this would

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bestimmt) der Raum oder die Zeit als Anschauungen zuerst *gegeben* werden, so gehört die Einheit dieser Anschauung a priori zum Raume und der Zeit und nicht zum Begriffe des Verstandes (§ 24).

[4] The tension is prima facie because it could be argued (Allais 2009, 396) that not all synthesis requires concepts and the understanding, and that the understanding is first required to bring the synthesis to concepts (B103/A78). That is to say, the synthesis of apprehension and reproduction, discussed in the A-Deduction, might be taken as not, or not directly, involving the understanding. However, we would argue that in view of Kant's main claim that the "same function," that is, the "same understanding," "by means of the very same actions," "gives unity to the different representations *in a judgment*" and "gives unity to the mere synthesis of different representations *in an intuition*" "by means of . . . synthetic unity" (B104–5/A79), and Kant's further claims that the synthesis of the imagination is "an effect of the understanding" (B152) and "depends on" the understanding (B164), it is hard to see how the act of synthesis can be seen as independent of the understanding, and thus as preceding the concepts of the understanding. So if, as Kant maintains, the unity of space precedes concepts (whether in general or the categories), then it cannot presuppose a synthesis.

contradict Kant's attacks upon the Newtonian view that there is such a thing as an object "space" (A39/B56). Also, in the Amphiboly of concepts of reflection, Kant refers to the representation of space as an "empty intuition without an object," that is, an *ens imaginarium* (B348/A292).[5] This would appear to exclude the possibility of space being represented as an object.

3. In the final sentence of the footnote, the unity of space is presented as belonging to space, and since space is an a priori intuition, this means that the unity is internal to the intuition. However, in the main text, at B161, we find Kant describing the "*unity of the synthesis*" of the manifold of space (or time) as "already given *a priori*, although with (not in) these intuitions." Since the unity in the footnote is also described as presupposing a synthesis, how can the unity of this synthesis belong to space, if this synthesis itself is not given in space? That is, the worry is that the unity of space is grounded in a synthesis that is external to space (according to B161), while this unity belongs to space (according to B160n), which suggests it does not involve reference to anything external to space. The extent to which the unity that belongs to space is not imposed on it through synthesis, and is therefore nonconceptual (since synthesis implies unification under a concept—see above), thus requires further examination.

As we shall see below, these issues are related to the distinction Kant introduces in the footnote between space as "form of intuition" and as "formal intuition." This distinction has been the subject of some discussion, in particular in response to Longuenesse's (1998a, 212–27) controversial claim that despite Kant's explicitly contradistinguishing them, the two notions can be fused into one. We shall argue that there are good grounds for keeping the two apart, although they are perhaps not as distinct as they are considered to be according to a more traditional reading, which takes the distinction at face value.[6]

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[5] See also Refl. 4673 (AA 17:638–39): "Space is not an object of intuitions (an object or its determination), but the intuition itself, which precedes all objects and [*crossed out*: through which] in which if the latter are posited [*und wodurch es worin die selbe gestellt*], the appearance of them is possible" (Kant 2005, 155). We note also that in the First Antinomy, Kant underscores the fact that space is "not a real object that can be outwardly intuited" (A429n/B457n).

2. Main Interpretative Strands

Let us start with the first issue of the contrast between the claim (A) that the unity of space is prior to all concepts, and the claim (B) that the unity of space requires a synthesis. A first interpretation emphasizes claim (B). This is the option chosen by those interpreters who claim that the unity of space is to be understood as produced by the spontaneity of the understanding by means of a priori concepts. We shall call this the *Conceptualist* interpretation.

The motivation behind this interpretation involves, first, simply arguing that the unity of space is the unity of a manifold of representations in intuition and that, as such, it must be a *synthetic unity.* Such a unity is understood by Kant as logically anteceding (in transcendental logic), that is, grounding, *analytic unity* (B133n). This is what Kant explains when he introduces the synthetic unity of apperception as the highest principle of all knowledge in the B-Deduction: "The *analytical* unity of apperception is only possible under the presupposition of some *synthetic* one" (B133). The analytic and synthetic unities of apperception are not two separate functions of thought, but are two formally separable aspects of one and the same function of the understanding (see B104–5). The analytical unity of apperception refers to Kant's well-known claim at the start of the main argument of the B-version of TD that I must be able to accompany my representations with an 'I think' (B131–32). The analytic unity of apperception is defined as the *sameness or homogeneity* of representations with regard to the 'I think' accompanying them. All my representations express the same feature of being accompanied by the common representation 'I think'. By contrast, the synthetic feature of apperception is the *combinatory* nature of the unity of consciousness that comprises the various, heterogeneous representations under the common representation 'I think'. Only to the extent that various representations are put together can they share the common representation

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[6] The standard understanding of the notion of formal intuition sees it as a conceptual determination of the form of intuition (Allison 1983, 97; 2004, 116; Falkenstein 1995, 383n31). Falkenstein writes, "Since, as B129–30 points out, all combination is an intellectual act, it naturally follows that the formal 'intuition' of space will be a discursive, intellectual representation ([that is, a representation relating to the understanding,] and hence not an intuition, properly speaking, at all)."

'I think' and can the 'I think' accompany them "conjointly" (Van Cleve 1999, 80), which accounts for the grounding of the analytic unity of apperception on the a priori synthesis of one's various representations. Thus, while "the analytical unity of consciousness pertains to all common concepts as such" (B133n), a synthetic unity necessarily precedes the analytic unity as its logical condition according to transcendental logic, that is, as a necessary enabling condition. For it is necessary that the multiplicity of representations be gathered by means of a synthesis to first ground a concept's analytic relation to the representations to which it applies. For example, the concept "red" can be applied to a multiplicity of red-colored objects on the grounds that those objects have been taken together as having the characteristic "red." It is thus that, according to Kant, the synthetic unity precedes any *concept*, to which by definition attaches an "analytic unity of consciousness" (B133n).[7] This is a statement that Kant makes about manifolds in general in the first part of the B-Deduction (secs. 15–20), where the manifold is considered independently of its specific spatiotemporal nature. The additional claim that synthetic unity requires a synthesis is made by Kant at B130–31. Kant shows here that there is a reciprocity between the representation of this synthetic unity and its product, namely, the combination of representations constituting the synthesized manifold. A combination in the manifold is made possible insofar as the representation of the synthetic unity has been "added" to the manifold, which requires what Kant calls a "higher" unity, that is, the original-synthetic unity of apperception.

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[7] More specifically, we read Kant's claim, at B133, that analytic unity of apperception or consciousness presupposes an a priori synthetic unity as a claim about the biconditional that analytic unity of consciousness in regard to my representations obtains if and only if representations have been synthetically combined in one unitary representation (which Kant calls the 'I think') by means of an a priori act of synthesis. A priori synthesis is the "antecedently conceived" (B133n) condition of the analytic unity of self-consciousness. It is thus a necessary condition of analytic unity of apperception, that is, "for me to represent the *identity of the consciousness in these representations* itself" (B133). But a priori synthesis is also a sufficient condition for the representation of the analytical identity of self-consciousness. For no *a priori* synthesis fails to result in an analytic unity of apperception, since considering my representations as belonging to my analytical identity of self-consciousness "means . . . the same as that I unite them in a self-consciousness" (B134) by means of an a priori synthetic unity. See further Schulting (2012b) for an interpretation of the relation between the synthetic and analytic unities of apperception.

Building upon this connection between synthetic and analytic unity, recently Hermann Cohen's (1907; 1987) neo-Kantian[8] interpretation of CPR has been revived in a paper by Éric Dufour (2003) that applies Cohen's general approach to the issue of interpreting the footnote.[9] Dufour claims that the unity we are after is that which makes geometry possible and produces the concepts of space and, as such, must therefore be the unity of the synthesis of space, that is, a synthetic unity. This unity, like the synthetic unity of the synthesis of any manifold, stands in reciprocal relation to the analytic unity of apperception, which it grounds (B133), and hence amounts to a conceptual unity. As Dufour shows, this interpretation has the benefit of explaining much of the content of the footnote in a fairly straightforward fashion. That is, the geometric unity presupposes a synthesis insofar as there must be a synthesis of space, which gives rise to it as synthetic unity. It thus apparently makes all concepts of space possible. This would seem to provide an explanation of claim (B).

Upon closer examination, however, this conceptualist interpretation can really only validly claim in this way that geometric figures (subspaces, or conceptual determinations of space) are the result of a synthesis (Dufour 2003, 77),[10] given Dufour's conceptualist reading that synthesis is strictly reciprocal to conceptual unity, and thus that syntheses always issue in concepts. On the assumption that synthesis is strictly reciprocal to conceptual unity, it cannot validly claim that space as a whole is the result of a synthesis, since space is described by Kant as not being a concept, in particular because it is infinite in magnitude (A25/B39–40).

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[8] Cohen was the founder of the Marburg school of neo-Kantianism, which was continued by Paul Natorp and Ernst Cassirer, among others.

[9] Cohen himself does not actually say much about the footnote (see, for example, Cohen 1907, 62), although Dufour (2003, 78–79) does not believe this reflects any unease on Cohen's part with regard to its compatibility with his overall interpretation. Although Cohen's interpretation of TAe as pointing forward to the Transcendental Logic is echoed in the reading that is proposed in this article, our interpretation diverges from Cohen's core claims about space. Cohen (1987, 141) asserts that space (and time) can be represented only as *quanta continua* because the understanding, by means of the productive imagination, "is active in them," and that space and time originate in synthesis (1987, 99). For Cohen, space as pure intuition is a scientific abstraction, when regarded as detached from the synthesis of the understanding (1987, 141). We shall see further below in what ways this interpretation is flawed.

[10] "It is indeed the understanding which 'produces' (*hervorbringen*) succession. . . . The same goes for space when the understanding is applied *via* inner sense to outer sense" (Dufour 2003, 77n; our translation).

That is to say, the content of a concept is characterized by Kant in terms of marks that are related to it as partial representations (AA 9:58). We would, however, not be able to complete a synthesis of an infinite number of such partial representations. Thus, while concepts are generated by synthesis of a finite number of partial representations, this cannot be the case for space, given that space is an infinite given magnitude. Space as infinite given magnitude cannot, therefore, be the *product* of a synthesis considered as strictly reciprocal to conceptual unity. However, Dufour (2003, 76) draws upon the use of the description "concept of space" in the Metaphysical Exposition (ME) of TAe as grist to his conceptualist mill insofar as it can be taken to imply that space (as a whole) requires a synthesis. That is to say, he recognizes that the representation of space is an intuition, but he follows Cohen (1987, 170) in taking Kant to have understood space and time as "*according to their concept*, not concepts, but intuitions" (Dufour 2003, 76; our translation and emphasis added).

The issue here, however, is the status of the concept (in the singular) of space as an intuition, and with respect to this, we can see that Dufour's strategy clashes directly with claim (A). For by identifying space as discussed in TAe as in effect involving the unity of a concept, he cannot also say of *this* concept, that the unity of space precedes it. And yet, the footnote claims that it is true for all concepts (in the plural) of space that the unity of space precedes them. We are therefore owed an explanation as to exactly what the unity of space might be on Dufour's conceptualist interpretation, such that it precedes *any* concept of space.

Not all conceptualists, however, fall foul of the above-mentioned problem, and indeed, it is noteworthy that Longuenesse has an answer to this question, which involves somewhat revising the standard conceptualist view. That is, Longuenesse (1998a, 241ff.) has it that the intuition of space is indeed produced by the understanding, but in a way that involves a role of the understanding that is prediscursive,[11] in the sense of being prior to the application of any particular concepts of space in a judgment about space, but not prior to the application of the categories. While her approach implies having to accept a controversial notion of an initial application of the categories of the understanding that does not entail subsumption of a manifold under the categories in a judgment, it is worth noting that (a) this finds its place in a well-argued overall interpretation of the capacity to judge, and (b) that Longuenesse is not alone in considering the need for a role for the understanding that is prior to concept subsumption. Indeed, Michael Friedman also identifies a prediscursive role for the understanding. He understands the representation of space as resulting from "an original synthesis [that] precedes all (schematized) categories or pure concepts of the understanding" (Friedman 2012, 248), where schematized categories must be understood as the categories functioning in an actual empirical judgment. Insofar as the understanding, by way of its action of original synthesis, is involved in the generation of the intuition of space, these approaches are "conceptualist" as we have defined the term, but it is important to note that on Longuenesse's and Friedman's accounts the understanding's role is considered prediscursive, that is, prior to any actual conceptual unification in an actual judgment. These latter interpretations are therefore described as "broadly conceptualist" as opposed to the Dufour/Cohen reading, which is "strictly conceptualist."

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[11] Fichant (1998, 92) describes the appeal to a "pre-discursive understanding" as an "interpretative artefact" that is "massively external to the Kantian thesis of the duality of the 'origins' of knowledge" (our translation).

As an alternative to the conceptualist interpretation such as Dufour's, one can emphasize claim (A), that is, the claim that the unity of space is prior to all concepts. This *Nonconceptualist* option is preferred by interpreters such as Arthur Melnick (1973, 11), Falkenstein (1995, 9), and Henry Allison (2004, 113; 2000; 2012).[12] As Allison (2004, 192) notes, the main support for such a move lies in other texts than the footnote (for example, in TAe), as the footnote is usually thought to pose problems for the nonconceptualist, with its emphasis upon the need for a synthesis. Indeed, the standard nonconceptualist response to interpretative problem 1 from the footnote is to insist that what Kant is referring to when he says that a synthesis is required for the unity of space is the need for syntheses in the construction of geometric figures, not for the unity of space as such. And the justification for this move lies in the opening sentence of the footnote, which indicates that we are indeed dealing with space "as is really required in geometry" (B160n). Michel Fichant (1997, 36–38), for instance, takes this line, but not without noting that there are terminological obscurities in the footnote. For example, the second sentence clearly states that it is the unity of space that somehow requires a synthesis, not the unities of geometric constructions. As with the conceptualist option, a clarification is called for as to what the unity of space discussed in the footnote amounts to.

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[12] Although the constraints of Jill Buroker's (2006, 130) book restrict her to a short section on the interpretation of the footnote, it is interesting to note that she clearly differentiates the unity given to space and time by the understanding from the unicity of space and time as forms of our sensibility. For similar reasons, Sebastian Gardner's (1999, 84–85) discussion is also brief, but suggests that he takes a nonconceptualist line too.

It is also worth noting that, as with Dufour's conceptualist interpretation (see above), there is again an equivocation over the use of the singular and plural: for the conceptualist, this concerns the *concept* versus *concepts* of space; for the nonconceptualist, the equivocation concerns the *unity* of space versus *unities* of spatial constructions. Indeed, for the nonconceptualist, the need for a synthesis can be explained for unities of spatial constructs but not for the unity of space. The demand for another account to explain in what sense a synthesis is needed for the unity of space *as a whole* is ignored by the nonconceptualists, whose interpretations discuss only the unities of parts of space. This equivocation over whether unities of parts or the whole of space are at stake parallels that characterizing the conceptualist interpretation insofar as, according to the conceptualist interpretation, what is clearly true for concepts of space, such as geometrical concepts (that is, that they are the result of a synthesis), is taken as therefore true of the concept of space (as a whole).

As with the conceptualist option, there is a more controversial interpretation that avoids the problem of this equivocation for the nonconceptualist. This option might be characterized as "radical" in contrast to the "traditional" nonconceptualist interpretation just described, and its chief exponent is Heidegger. Heidegger's (1991; 1995) radical interpretation gives pride of place to a faculty of the imagination that is no longer simply serving the faculty of the understanding, but is autonomous. This interpretation is justified by appealing to the central role Kant attributes to the imagination in the A-version of TD. Since problem 1 is that of explaining how the unity of space can presuppose a synthesis while making all concepts of space possible, if, with Heidegger, one views the synthesis referred to in the footnote as the sole work of the imagination, and not of the understanding, this problem disappears. Heidegger's interpretation of Kant is generally considered as involving a selective attitude toward the available textual evidence. But it is nevertheless of philosophical interest in its potential threat for the Kantian project, as we shall see later (section 3.G).

The options in terms of the interpretation of the second sentence of the footnote can thus be summarized in the following table in which we also place the interpretation we shall present below:

Conceptualist about unity of space		**Nonconceptualist about unity of space**	
Strict:	*Broad:*	*Orthodox:*	*Radical:*
Cohen	Longuenesse	Melnick	Heidegger
		Falkenstein	
Dufour	Friedman	Allison	
		Fichant	
		+ This article	

The placement on the left reflects a greater role for concepts. Many more authors could, of course, be added to this table, but those included can be taken as paradigmatic for each category.

While this classification is based upon the stance taken toward problem 1, this has repercussions for problems 2 and 3 respectively. Recall that problem 2 is that of understanding how space can be considered an object, while problem 3 is that of understanding in what sense the unity of space belongs to space. As regards problem 2, the strict conceptualist must unequivocally endorse the thesis that space is an object insofar as space is, after all, a product of the understanding derived from a concept of space, given that Kant defines "object" as the objective unity of one's representations (B137), which he equates with the transcendental unity of apperception (TUA) (B139). Conversely, concerning problem 3, the nonconceptualist is committed to viewing the notion of the unity of space as belonging to space, since it is, after all, a nonconceptual unity.

Leaving aside any further examination of problems 2 and 3 at this stage (we return to them in section 8), we will now turn from a brief review of the geography of the interpretative positions to a first examination of the "unity of space" as required by the issues raised by both conceptualist and nonconceptualist options, and examine to what extent these options are able to account for it.

3. Which Unity of Space?

A. "Unity" in TAe

In the footnote, Kant addresses the unity of space and claims that "in the Aesthetic, I ascribed this unity merely to sensibility" (B160n). What then is this unity supposedly discussed in TAe to which Kant refers here? In fact, reading the Aesthetic, we do not find any *explicit* use of the term "unity". We therefore look for *implicit* references to unity, first in the Transcendental Exposition (henceforth TE) of the concept of space

(B40–41), which identifies the "transcendental function" (Gardner 1999, 121) of space, that is, its role as grounding some form of a priori knowledge. In that section, however, all we find is that geometrical knowledge is grounded in the fact that the representation of space must be an intuition and must be a priori (B40). That the representation of space possesses these characteristics is, however, what ME set about to show, by exhibiting in what way space, understood as an a priori intuition, can provide the ground for geometry. This suggests that if there is a notion of unity to be found in TAe, it is in ME.

B. "Unicity"

The unity we are after is however such that it "precedes all concepts" (B160n). Geometry, on the other hand, like any other form of objective knowledge, involves conceptual determinations in the sense that geometrically determinate spaces are conceptual determinations of space. Geometrically determinate spaces do not have a unity that "precedes all concepts" since they require geometric concepts (which are a priori) for their determination, which occurs by virtue of the construction of geometric objects (A162–63/B203–4). The unity of space that precedes all concepts in TAe cannot therefore simply be the unity of a geometrically determinate space. Nevertheless, if the unity at issue characterizes space, it also has a role to play in grounding the possibility of geometry.[13]

Importantly, geometry is also mentioned in ME, a fact which is prima facie puzzling as Kant clearly wants to separate the presentation of the nature of space in that section from an examination of its transcendental role in the next. We shall return to this issue later below. For now, let us note that in ME, Kant argues that space is *single and prior to its parts*, and therefore is an intuition rather than a discursive concept (B39/A25; compare B136n). What Kant means by that is that any determinate space can only be represented by reference to a single prior indeterminate space of which it is a part. This dependence is not characteristic of concepts, which are obtained by synthesis of manifolds of representations, that is, defined by reference to their parts or partial conceptual representations or marks (AA 9:58).

Insofar as, for the transcendental idealist, space is nothing beyond the representations of space (A28/B44), this priority of the whole over the parts is characteristic of space: the fact that I can represent a region of space only by reference to the whole of space implies that space as a whole is a prior condition of the possibility of something like a region of space. We note that there are independent grounds for holding such a view about space, namely, monism about space, according to which the whole of space is metaphysically prior to its parts (Schaffer 2010, 35–36). For the sake of brevity, and to refer to the different order of priority characterizing conceptual representations, we shall call "mereological inversion" the property an entity has insofar as any representation of a determinate part of it depends upon a reference to the whole of it. On this definition, space possesses the property of mereological inversion. Mereological inversion contains a notion of "unity of space," although it is not spelled out explicitly: it is insofar as parts of space share a common origin in the "single all-encompassing space [*einigen all-befassenden Raume*]" (B39/A25) that they can be said to form a unity.[14] This unity is not that which Kant identifies in the Analytic of CPR as a condition for knowledge, as it is not that unity established through a synthesis, that is, a *synthetic* unity. On the contrary, it is first characterizable as the *analytic* unity of that which is unique: since there is only one space, all spatial regions therefore share a common origin; they analytically share the *same* characteristic—spatiality. We understand by "spatiality" the set of properties of space that characterize its unity. Similarly, any manifold in space is related to any other manifold in space precisely through its belonging to the same unique space. This is the unity of what Kant calls a *totum analyticum* (Refl. 3789, AA 17:293).[15] An important feature of this unity is that it implies *infinite divisibility* as no limit can be imposed on the process of determining parts. Moreover, this unity is that of something that is given as *infinite*,[16] hence not complete, while it would seem that a synthesis, as understood in the context of the analysis of discursively possible knowledge, can bind only a completed set of representations, as they must be apprehended together and reproduced by a spontaneous finite act of thought (A100–2).[17]

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[13] It would be a ground that would serve as foundation for any representation of spatial manifolds, whether geometrically determinate or not.

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[14] We note that the German *einigen* could be translated as "unitary" here, in contrast to *einzigen*, which would more properly be translated as "single". See our translation of Kant's text *On Kästner's Treatises* (Kant 2014). See also further below.

[15] The analyticity referred to here is not conceptual, but that of an intuition. As Cohen (1907, 29) notes, "this unity [*Einigkeit*], out of which the manifold unfolds, goes beyond the methodology of the concept" (our translation).

[16] Fichant (1997, 33–34) provides an illuminating analysis of space as a "quantum," and moreover an "original" quantum (as opposed to a determinate one), which he contrasts with the notion of *quantitas*.

To summarize, the unity of space in question is characterized through the properties of:

1. *singularity*: one can only represent a single, unique space;
2. *mereological inversion*: the whole of space precedes its parts (which entails the property of *infinite divisibility*);
3. *infinity*: the magnitude of space is infinite.

These three properties of singularity, mereological inversion, and infinity characterize space as a *unicity*, the term we shall use to indicate the sui generis unity of space. Although these are metaphysical characteristics of space, they have a transcendental role in cognition, which Kant spells out for instance in Refl. 4673 from the *Duisburg Nachlaß* from the 1770s. There, Kant claims that "the capacity to receive several impressions of outer objects, or the susceptibility to them, has no limits in itself" (AA 17:641 [Kant 2005, 156–57]). That is to say, space enables the representation of pluralities of representations of external objects whatever they may be, that is, however large or fine grained. But there is more to this transcendental role: the unicity of space defines a *structure of space* that characterizes the relations between regions of space. Kant presents space as the condition for the representation of things as "outside and next to one another" (A23/B38), which defines the structure of space in terms of the notion of exteriority (we shall say more about this further below).

In most of CPR, Kant retains the emphatic use of the word "unity" for synthetic unity, and thus it makes sense to use a distinct word, "unicity", for what is discussed in TAe concerning the unity of space.[18] This unicity appears to be the only plausible candidate for the unity we are after and to which Kant refers in the footnote. Moreover, Kant himself, in a response to an article by the mathematician Abraham Kästner on the representation of space, refers to the parts of space as belonging "to one space [*ein* (*einiger*) *Raum*]" (AA 20:419), that is, to a space that is single or *unitary* (*einiger*), whereby the unity in question, which we call unicity, is that of "eines eintzigen [Raum]" (ibid.), that is, that of a *unique* (*einzigen*) space. Friedman (2000a, 198) underscores this notion of a "distinctly intuitive" unity of space that is not conceptual (compare Kjosavik 2009, 19).[19] We also note that Georg Mohr (1998, 112–13), who examines the case of time, identifies an analogous notion of temporal unity in TAe. It is this unicity of space that Kant identifies in his analysis of the impossibility of constructing a figure enclosed in two straight lines as arising from "the conditions of space and its determinations," and not from "the concept in itself" (A220–21/B268).[20] That is to say, it is the nature of spatiality (here, its topological features) independently of any synthesis of space under the categories that is at stake here.

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[17] By contrast, concepts, whose extensions may be infinite, are finite with respect to their intension insofar as they have a finite set of marks. The search for further subordinate concepts (that is, concepts with additional marks defining the extension of a given concept) can be pursued ad infinitum, as Kant explains in the Vienna Logic: "For someone who accepts *species infima*, these *subdivisiones* will finally have an end. Since we have just shown, however, . . . that in the nature of the thing every *species* can always contain further *species inferiores*, we will never come upon such concepts, whose sub-contained concepts cannot be divided again. Subdivision must proceed to infinity, then, although many a subdivision has an end, of course, *comparative*, for us" (AA 24:927 [Kant 1992, 368]). But the important point is that when we turn from extensions to consider intensions, we find that an infinite concept cannot be grasped by the human mind (compare Allison 1983, 93).

[18] Prior to CPR, Kant appears to use "unity" explicitly in connection with space at least on one occasion. In a *Reflexion* that is probably part of the *Duisburg Nachlass* (Refl. 4756, AA 17:700) and presents a sketch of the later argument of TAe, Kant suggests that

C. *The Conceptualist Reading of the Unity of Space*

However, evidently the nature of the investigation in TAe is such that it is only the contribution of sensibility that is at stake there. By contrast, the Analytic's examination of the contribution of the understanding to cognition shows that insofar as cognition should arise from it, every sensible manifold must be synthesized under TUA, that is, under the principle of the original-synthetic unity of all of a given subject's representations that must be able to be accompanied by an 'I think' (B131–32).

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unity, which he gives as one of the characteristics of space, entails that space is an intuition and not a concept of the understanding: "Space . . . 5. Unity, hence [*mithin*] a pure intuition and not a concept of the understanding" (Kant 2005, 181). To avoid confusion between spatial unity, which is not a concept of the understanding as made clear in the above passage, and a unity that *is* grounded in the understanding, we use *unicity* for the former unity.

[19] Longuenesse (1998b, 83; 2005, 36) also highlights this unicity, but for her both the unity and unicity of space are brought about by the understanding, whereas we understand space's unicity as precisely *not* brought about by the understanding but rather as characterizing space essentially.

[20] Importantly, both the "conditions of space" and its "determination" play an explanatory role here. This leaves room for a non-Euclidean geometry with different determinations of space (see note 73 below).

The relevant issue here between the conceptualist and the nonconceptualist is that of the relation between the two types of unity, that is, the unicity of space and the unity that is imposed on space by TUA. The conceptualist would have it that there is only one notion of unity in Kant, namely, the unity defined by TUA, which as we have seen with the conceptualist approach, is a synthetic unity that is coextensive with a conceptual (analytical) unity. As a result, on the conceptualist approach, it must be the case that the unicity of space is a product of TUA,[21] so that the unity described in ME, although originally presented as being ascribable to sensibility, is, as the footnote allegedly reveals, in fact ascribable to the understanding. The problem this approach raises is that it is not clear whether it can, after all, satisfactorily explain the second sentence of the footnote. Indeed, if the unity of space requires a synthesis and every synthesis is dependent on TUA (B130–31; B131–36), the unity of space is dependent upon TUA. Consequently, given the reciprocity between TUA and conceptual unity, the unity of space is defined by a concept, namely, the concept of space (by which Kant means a concept of that which is given in the intuition of space), which is presented in ME. The content of this concept is the set of characteristics of the intuition of space as they are given in ME. But then it is not clear why this unity could be said to *precede* all concepts of space. This is not only, as observed earlier, because the very concept of space from TAe would be one such concept. But it is also because it must be possible to define conceptually any region of space, if space as a whole is so defined. So why would the unity of space that is defined by the whole of space be needed to define a region of space? All that is required to define such a region, if the logic of this conceptualist approach is adopted, is to add to the general concept of space some marks defining the limits of the region in question. Nonconceptualists will of course have an answer to that question, as we shall see below, as they can draw on the properties of the unicity of space.

D. *Longuenesse's Middle of the Road Approach*

The problem flagged above with the conceptualism of the Marburg school interpretation, which Dufour defends, can however be obviated without abandoning the thrust of the conceptualist approach, namely, by retaining a role for the understanding in defining the unity of space as it is identified in ME. This is Longuenesse's approach. Longuenesse does not in fact construct her interpretation of CPR centrally around her understanding of the footnote. It is therefore all the more remarkable that her interpretation should provide her with a coherent understanding of it (Longuenesse 1998a, 215–25).

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[21] As is indeed claimed by Longuenesse. See note 19 above.

As we have noted earlier, Longuenesse (1998a) proposes to introduce a prediscursive role for TUA, whereby the unity of space results from a prediscursive "determination of sensibility by the understanding" (1998a, 222–23). Longuenesse's proposal follows from her broader proposal for the interpretation of the guiding thread that Kant presents in the Metaphysical Deduction of the categories (MD) (see B104–5). This leads her to interpret the role of the understanding in its effect upon sensibility as involving the capacity to judge as a precondition for "the *analysis* of the sensible into empirical concepts" (Longuenesse 1998a, 64). That is to say, the sensible manifold is compared, reflected upon, and abstracted by the understanding so that concepts can be formed on the basis of this procedure, which can subsequently be discursively employed in judgments. This effect of the understanding upon sensibility, which Kant says is the "first application" of the understanding (B152), is brought about through the imagination's *synthesis speciosa,* that is, the transcendental synthesis of the imagination, in such a way that the manifold in sensibility be available for subsumption under a concept according to the categories (Longuenesse 1998a, 244). For Longuenesse, such a subsumption need not happen however: if it were to happen, it would involve a second synthesis, the *synthesis intellectualis* (1998a, 245).

What are the implications of her reading for the intuition of space? Longuenesse's differentiating of a first prediscursive synthesis (1998a, 72) from any conceptual determination through a second intellectual synthesis (which happens in the context of discursive judgment) leads her to assign to this first prediscursive synthesis the function of *generating* the pure intuition of space (1998a, 219). In Longuenesse's view, space and time are therefore "the first, most original 'effect of the understanding on sensibility'" (1998a, 223; compare Longuenesse 2000, 103).[22] For our immediate purposes, it is important to note that on Longuenesse's reading the synthesis that generates the unity of space does not involve a concept of space. Longuenesse (1998a, 224) would argue, in line with the footnote's claim, that unity precedes all concepts. However, for Longuenesse, the role of the understanding involves the functions of judgment since the understanding is a "*faculty for judging*" (A69/B94), and as such the categories, being nothing but these functions of judgment insofar as the determination of empirical intuitions is concerned (B143), are already implicitly involved, namely, as mere functions for judging (Longuenesse 2000; 2005). This is however an application of the categories in a Pickwickian sense, namely, in a merely guiding role,[23] while the imagination plays the key role in the logical process of comparison, reflection, and abstraction, as Longuenesse argues.

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[22] This is confirmed by Longuenesse's (2000; 2005, 29–37) replies to Allison's (2000) critique of her position. Space and time, Longuenesse (2005, 34) states, "are forms of a sensibility affected by the understanding, and thus they are the product of *synthesis speciosa,* the transcendental synthesis of imagination."

With this interpretation of the *synthesis speciosa* as that which first generates space, there seems little "spatial" content left in Longuenesse's notion of space. With Longuenesse's interpretation, sensibility is given a key role, but not one that reflects the phenomenology of space, the *spatiality* of space so to speak (compare Buchdahl 1969, 579–82). While Longuenesse (2000, 104; 2005, 34) claims that "the qualitative features of spatiality and temporality depend on our sensibility," she has nothing more to say about these. Correlatively, it is difficult to see how Longuenesse's interpretation can accommodate the actual infinity of space, although she claims that it does (Longuenesse 1998a, 267). Indeed, if the understanding were to produce space by affecting sensibility, it is not clear how an actual infinity is a possibility here: the understanding can certainly have no grasp of such an infinity, let alone be responsible for the imagination's ability to generate it. Here, it seems that Longuenesse is actually less sensitive to the problem of infinity than the neo-Kantians of the Marburg school. The latter at least recognize a role for infinity in cognition insofar as their notion of empirical object is defined as the limit of an infinite sequence of successive determinations.[24] Similarly to Longuenesse's, Friedman's (2012, 246–49) account of the footnote does not involve a concept of space either but, as we noted before, a role for the understanding that is prior to the schematization of the categories.

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[23] The guiding role of the categories consists in their "guid[ing] the ordering of our representations of... objects so that we can form concepts of them and combine those concepts in judgments" (Longuenesse 2005, 82; see also Longuenesse 2005, 24 and 1998a, 123). Longuenesse distinguishes between two stages of application of the categories: the first stage concerns the noted guiding role of the categories, so that by means of the process of reflection, comparison, and abstraction, representations are first conceptualized for use in judgments, and the second stage concerns the effective application of the categories in judgment, that is, the subsumption of objects under categories in a judgment (Longuenesse 2005, 24–26). Notice that, according to Longuenesse, the categories do not just guide representations of objects in intuition with a view to forming concepts of them, but the categories themselves are also first generated by the same process of reflection. This view of the categories is interpretively highly controversial. For a critique, see Allison (2000; 2012) and Schulting (2012b, 33–38).

E. The Conceptualist Approach and the "Unicity" of Space

It is not clear however that conceptualist strategies—even the more promising broad conceptualism of Longuenesse and Friedman—are able to account for the role that the unicity of space plays in our cognition. We noted above that in TAe the reason for Kant's introduction of the issue of geometrical knowledge in ME was not clear, particularly since the B-edition of CPR introduces the distinction between metaphysical and transcendental expositions, but retains the mention of geometry in the former. Looking at the sentences in TAe preceding the introduction of geometry is, however, instructive. Kant says that space is "essentially single" and that "the manifold in it, thus also the general concept of spaces in general, rests merely on limitations" (B39/A25).[25] "From this," says Kant, "it follows that in respect to it an *a priori* intuition . . . grounds all concepts of it." The next sentence then explains that "thus . . . all geometric principles . . . are . . . derived *a priori* with apodictic certainty" (ibid.). This suggests that the fact that space is single is relevant to its being able to ground concepts (in particular geometrical concepts) of it. This single space is then implicitly assumed as given in TE when Kant says that "geometry is a science that determines the properties of space" (B40), that is, of the *one* space, but this property of singularity is not properly explained by a conceptualist approach for which the unity of space is generated by the understanding. What would be lost in terms of objective knowledge of space without this property of singularity? We note two problems in particular:

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[24] We note however that, in her treatment of the principle of complete determination, discussed by Kant in the chapter on the transcendental Ideal (esp. A581–82/B609–10), Longuenesse does acknowledge, in a similar fashion, the distinction between, on the one hand, the indeterminate whole of spatiotemporal reality, as an infinite sphere of the concept of a spatiotemporal object (an expression used by Longuenesse, which reveals her strong affinity with the neo-Kantian school) and, on the other, all appearances, that is, actual spatiotemporal objects, as its determinations. Longuenesse (2005, 223) says that what is at issue here concerns "the whole of reality that grounds the representation of the complete determination of things [and] is the indeterminate whole of reality given in space and time, presupposed in any empirical use of the understanding giving rise to discursively represented *realities* or positive determinations of things (as appearances)." Thus, the determination of any possible object in space and time is the division of the infinite sphere "of the concept: 'object given in space and time', that is to say, 'object of experience'" (Longuenesse 2005, 218).

[25] Compare Refl. 4212, AA 17:458–59; A169/B211.

(i) Without it, geometry would have to deal with multiple non-interacting spaces, while it is clear that our science of geometry always operates with the assumption of a unique background space. That is to say, a priori spatial representations can only ground geometrical constructions insofar as such constructions can be carried out without requiring different spaces. Further, our objective experience is that of *one world of outer sense.* That is, objectivity requires the singularity of the whole domain of outer sense. Without this singularity, we could indeed have a form of objectivity, but it would be one in which we would simultaneously view ourselves as belonging to different worlds or domains of outer sense that are completely independent of one another.

(ii) At the level of empirical knowledge, what would be lost is also our understanding of the *particular* in outer sense. This touches upon the meaning of Kant's concept of the "transcendental object," which "concerns nothing but that unity which must be encountered in a manifold of cognition insofar as it stands in relation to an object" (A109). With several spaces, the object could be separated into an irreducible multiplicity in the different spaces, because there would be objective spatial determinations in each space. These could not be viewed as belonging to *one* object without assuming that these spaces are related, that is, that they all belong to some underlying unique "meta-space."[26] By excluding this possibility, Kant in effect takes it to be a feature of objectivity that there is a unique world of outer sense (compare Brandt 1998, 93). So the singularity of space is the unity of space as a *particular,* namely, as the form of outer sense, being the condition of possibility of representing an object. But conceptualist interpretations cannot explain how such particularity is obtained from the universality of a concept of space or of any

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[26] This would also have important ethical consequences as I, as agent, would belong to distinct worlds, and my behavior could, in principle, be distinct in these different worlds.

> other operation of the understanding, since the understanding is the faculty of concepts, that is, of the general or universal: it can specify from a formal point of view that space is a particular, but that does not provide us with a representation of the particular.

But aside from the singularity of space, the whole structure we have defined as the unicity of space, through which geometric constructions are made possible, is required, as Emily Carson (1997, 508) shows, to provide a ground for the axioms of geometry: "Construction in pure intuition reveals the form of (really) possible objects of empirical intuition, thus of objects of possible experience." And this is what the objectivity of geometry amounts to. What Carson identifies here is a notion of possibility defined in terms of construction in pure intuition and one that is to be differentiated from logical possibility. Only Euclidean geometry, or at least a geometry that is approximately Euclidean so that the differences at our scale are not perceptible, is possible on this understanding of possibility. Again, it is a notion of *particularity* that is at stake here: the world of outer sense has a particular form, that is, the form of the particulars in it, and this form is specified by the axioms of Euclidean geometry.

F. A Further Problem for the Conceptualist Approach

Finally, there is a systematic reason for doubting that a conceptualist approach to the notion of the unity of space does justice to this notion. Indeed, it is no coincidence that the issue of the unity of space is discussed in a footnote to the second part of the B-version of TD, the so-called "second step" of the B-Deduction. Without entering into the details of the complex issue of the structure of the argument of the B-Deduction, we assume it is fairly well established that it has two parts (Henrich 1969; Baum 1986; Evans 1990; Allison 1996), and that the second part, unlike the first, refers to the particular spatiotemporal nature of our human sensibility (B145). If the second part of the argument of TD is required, there are strong grounds for assuming that it is because the structure of space itself is not merely the result of a synthesis of any arbitrary manifold in intuition brought about by the spontaneity of the understanding. Rather, the issue seems to involve a recognition that since our intuition has a spatial form, the special question of this form's conformity to the categories arises, which thus calls for a deduction that legitimates the use of, not only the latter (which occurs in the so-called first step), but also the concept of space itself in the second step (see B120/A88).[27] We understand the very proof-structure of the B-Deduction as making sense only insofar as space is recognized as having a structure that is *independent of* the synthetic unity established by the categories, namely, as having its own unicity, so that the problem of unification under the categories of any manifold given within such a structure arises as a further question for the second part of TD. This second step can, in particular, be viewed as relevant to the possibility of geometry. Indeed, while TAe has shown that space as a priori intuition is a necessary condition for the possibility of geometry, this does not provide all the conditions for the possibility of this science. As with all knowledge, there is a role for the understanding, and here it must be its role to bring space under the categories, so that the construction of geometric figures is made possible. If the unity of space that is at issue in TAe were already that of a categorial synthetic unity, as Dufour claims, then there would be no task left for a deduction of the concept of space in the second step of TD.[28]

G. *The Nonconceptualist Reading of the Unity of Space*

Recognizing the transcendental function of the unicity of space thus strongly suggests viewing the unicity of space as being in essence nonconceptual. Unicity is a feature of space as the form of intuition. This leads some commentators to single out the unicity of space as having a key epistemological role. Thus Fichant (1997, 22–25) emphasizes the radicality of Kant's idea of the notion of an "original representation of space" (B40) in TAe that is precategorial. Fichant (1997, 24–25) also puts emphasis upon the independence of TAe from the Transcendental Analytic. But in terms of the footnote, what is prima facie puzzling for the nonconceptualist is that a nonconceptual unity could be claimed to presuppose a synthesis, while synthesis (in Kant's sense) must be taken to be strictly reciprocal to conceptual (analytical) unity (see again section 2 above).

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[27] In TD, Kant claims that time (and space) are indeed brought under the categories by the transcendental synthesis of the imagination (B151), and as a result, all that is in time (and all that is in space) also falls under the categories (B150). How time is brought to the categories is an issue that is addressed in the chapter on Schematism (A137–40/B176–80), but Kant does not say anything explicitly about space, a point we shall discuss further below.

[28] In Longuenesse's case, it seems her reading of the *synthesis speciosa* as generating space, by means of a first application of the categories in their guiding role, which only in a second instance requires an intellectual synthesis that results in a judgment, appears in fact to reverse the order of the proof in the B-Deduction, which by contrast moves *from* the argument concerning intellectual synthesis in the first step *to* the argument concerning figurative synthesis in the second step (compare Allison 2000; 2012).

Here, we should note that some commentators appeal to a substantial alteration of Kant's views between the publication of the A-edition and that of the B-edition. That is to say, the A-edition of TAe in which the sections do not carry titles featuring the word "concept" would present a nonconceptual notion of unity of space, while the B-edition introduces the notion of the "concept" of space (B37; B40), with Kant changing "we will consider space first" (A23) into "we will expound the concept of space first" (B38), as Reinhard Brandt (1998, 91) points out. Brandt takes this to mean that the B-version ensures that the unity of space is ultimately guaranteed by the understanding, *instead* of being the nonconceptual unity that characterizes the A-edition.

That there is a shift in text between the A- and the B-edition in this regard is clear. But whether we should interpret this as meaning that Kant changed his mind on this issue is another matter. Indeed, the passage discussed above about the unicity of space (B39/A25) remains unchanged in the B-edition TAe. So there is a notion of spatial unicity that is nonconceptual even in the B-version of TAe. If this is indeed the unity that Kant is discussing in the footnote, the issue of reconciling this with the claim that it "requires a synthesis" still has to be addressed.

Heidegger (1991) argues that the unity in question cannot belong to the senses if it is to involve a synthesis, and that since it is not conceptual, it must therefore be assigned to the imagination viewed as an independent faculty.[29] The imagination thereby acquires a central role that serves Heidegger's radical aims of enlisting Kant as a forerunner of his notion of finitude: through this notion, Heidegger wants to deny traditional notions of transcendence for human beings, for example, our claimed ability to know timeless truths. Indeed, for Heidegger, the understanding does not provide us with an ultimate objectifying principle that lies beyond the conditions of time (such as the unity of apperception), but rather, it is the imagination that ultimately governs the process of

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29. Heidegger (1991, 142) supports his interpretation by drawing attention to Kant's use of the term "synopsis" to refer to the function of sensibility in apprehending a manifold (A94). He does not, however, seem to fully appreciate the implication of Kant's not using the word "synthesis" here, as we shall see later. In particular, this synopsis is clearly carried out by the senses, not the imagination. Heidegger (1995) however introduces a new term "syndosis", which represents a function of the faculty of sensibility. On these issues, see further note 42 below.

cognition as the "mediating center" between the faculties of understanding and sensibility. Since Heidegger (1991, 176) interprets the imagination as having an "inner temporal character," this concurs with his understanding of the finitude of *Dasein* as temporality (Heidegger 1976, 234).

Heidegger's interpretation can, among other things, be criticized for bestowing an overinflated status upon a tripartite division sensibility-imagination-understanding. Although the A-Deduction (which Heidegger focuses upon) has Kant saying that "we therefore have a pure imagination, as a fundamental faculty of the human soul, that grounds all cognition *a priori*" (A124), he also states, further in the same paragraph, that "on them [that is, the categories] is grounded . . . all formal unity in the synthesis of the imagination" (A125). The imagination's synthesis is therefore governed by the categories, and hence by the understanding and TUA (A119). So even if the A-Deduction presents a tripartite division of the faculties "*sense, imagination,* and *apperception*" (A115), the latter two are intimately connected, with TUA providing a ground for the synthesis of imagination through the categories.[30] The introduction of the imagination into the debate may be a fruitful move, as we shall see, but Heidegger's interpretation, which views it as independent of the understanding, should thus be rejected on Kantian grounds. For Kant, any spontaneity that apparently belongs to the imagination is only the spontaneity of an apperceiving subject in conformity with the categories and thus with the pure understanding (A119; compare B151–52 and B162n):[31] a synthesis a priori brought about by an autonomous faculty of imagination is excluded by the roles Kant apportions to the cognitive faculties.

One might nevertheless worry that this Kantian critique of Heidegger's proposal ultimately relies upon a model of cognition that can be challenged on philosophical or systematic grounds. The challenge would be that if intuition and concept are indeed distinct sources of knowledge, the unity of space, and even the synthesis grounding it, could be argued to be independent of the categories. Without pausing to examine the possible grounds for such a challenge, let us note its implications. TUA, through which any given manifold acquires objective status, would have to "harmonize" with this independent unity of space.[32] As we have just seen, in Heidegger's interpretation, the ground of the unity of space is located in an autonomous faculty of imagination.[33] The contingency of the resulting knowledge announces the "tearing apart," as it were, of the Kantian a priori, which is meant to be the ground of the objectivity of knowledge: if some harmony is required between these two sources of knowledge, which itself is not guaranteed by a higher necessary principle, it is no longer possible to provide a necessary ground of empirical knowledge. This is certainly a consequence that is germane to Heidegger's philosophical aims: the *a priori* would then serve rather to indicate in what ways human cognition is constrained by contingent conditions, and this would be one feature of our essential *finitude.*[34] It follows that, aside from being very questionable as an interpretation of Kant's text, Heidegger's interpretation is not attractive as an account of Kant's project as a whole. If this brief excursion into Heidegger's writings on Kant has not provided a satisfactory account of how the unity of space could presuppose a synthesis, it has thrown up a problem for the nonconceptualist, namely, that of accommodating a unity of space that is independent of TUA, while not threatening the latter's unique role as the highest principle of all knowledge.

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[30] It is also the case that the imagination and sensibility are intimately connected ("the synthesis of the imagination, although exercised *a priori*, is nevertheless always sensible" [A124]), but this issue will be discussed further on in the article.

[31] On this point, we agree with Banham's (2006, 127–30) critique of Heidegger.

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[32] The worry here would be that TUA may not be compatible with the unity of space: this could mean either (i) that different regions of space could not be brought together into one realm of objective knowledge, or, viceversa, (ii) that TUA refers to something that cannot belong to space. These may seem like far-fetched possibilities. In fact, (i) is the case of an essentially heterogeneous nature with local laws only, while (ii) could be instantiated by the case of a nature that can be grasped only by reference to some nonnatural, for example, supernatural, entities or causes. The problem is that such possibilities could not be excluded, which would make knowledge contingent upon their not actually being instantiated. Although the first case seems implausible, note that Heidegger's criticisms of the Kantian/neo-Kantian project in connection with time amount to the denial of the very possibility of something like eternal laws (Heidegger 1991, 277–78). Rather, for Heidegger we are located in our finite temporality, so that the very notion of the unity of time as defining an eternity is not within our grasp. Although Kant does not consider such cases of discrepancies between the unity of space and TUA, the worry they give rise to is analogous to that which Kant voices when dealing with the reproductive function of the imagination. He points out that "if this unity of association did not also have an objective ground . . . then it would also be entirely contingent whether appearances fit into a connection of human cognitions" (A121–22). In effect, such a connection would rely upon a contingent harmony, which would invalidate any claims about necessary knowledge.

[33] The unity of space could then be understood in terms of an impoverished notion of spatiality, where the latter is defined in terms of what Hubert Dreyfus (1991, 99) calls the "nexus of equipment."

[34] The story is presented in a very summary form, but the key idea is that any claim to necessity is invalid, since contingency pervades all knowledge because of our reliance upon the entirely contingent conditions of sensibility. We take it to be Kant's singular achievement to have made sense of the role of the nonconceptual in knowledge or experience and to have assigned it a role in the constitution of the a priori. Compare the problems that Heinrich Rickert (1909) and Emil Lask (1912) ran into without a notion of the nonconceptual, when trying to reconcile the realm of values with the realm of reality, and how this led to Heidegger's formulating his original ideas about the analytic of *Dasein* (see Friedman 2000b, 35–37).

4. The Need for a Synthesis and the Distinction between "Formal Intuition" and "Form of Intuition"

The dialectical situation is the following. So far, we have identified an analytical notion of unicity of space in TAe. While conceptualist approaches (Cohen, Dufour, Longuenesse, Friedman) can account for how it is that the unity of space requires a synthesis, they do not appear able to account for the epistemic role of a unity of space, as described in TAe, namely, a unity that precedes all concepts.

The nonconceptualist approach is, however, faced with two challenges, namely, that of explaining how the unity of space presupposes a synthesis and how it stands with respect to TUA. Rejecting Heidegger's radical nonconceptualist interpretation leads us to develop a nonconceptual interpretation that draws upon existing traditional nonconceptual approaches (Allison and Fichant in particular). Its key feature is to provide a novel reading based upon the observation that the two challenges we have flagged are interrelated. In presenting this interpretation, we shall, in particular, contrast it with the broad conceptualist approach that emerged as the more plausible conceptualist interpretation from our analysis in the previous section.

In discussing the key claim that the unity of space "presupposes a synthesis" (B160n), we have so far assumed that Kant is describing the unicity of space (as presented in TAe) as requiring a synthesis, for without a synthesis there would be no such unity (and it should be clear that an *a priori* synthesis is meant here). This understanding of "presupposes a synthesis" however overlooks that the introduction of a dependence relation here necessarily refers to the faculty of understanding. That is, what is at stake is *the grasp of the unicity of space by the faculty of understanding*. What one usually fails to see here, however, is that "to presuppose" does not amount to "to be generated by." As often in interpreting Kant, the danger of confusing a justificatory with a genetic issue rears its ugly head here. Allison, who is particularly attentive to these matters, stresses in another context the importance of the "as" structure in differentiating such issues (Allison 1990, 37). And indeed, the footnote clearly states that the topic at stake is that of space "represented *as* object" (emphasis added; compare Allison 2012, 48). Leaving aside for the time being the issue of how exactly to interpret "object" here, what is clear is that what is at stake is space, in particular the unity of space, *insofar as* it is grasped by the faculty of understanding.

To clarify our interpretation of this claim, consider the statement at the start of TD in its B-version, namely, that "the I think must be able to accompany all my representations" (B131), the principle of TUA. This proposition does not mean that any representation whatsoever has to fulfill this requirement. Only those representations that I *take as my* representations need ever be accompanied by my 'I think'. A strict reciprocity obtains between the indexicals 'I' and 'my'. Any other representations of which I am not occurrently conscious or of which I could not be conscious are, as such, not representations that 'I' (*stricto sensu*) represent; that is, these representations are or could be *represented*, but they are not thereby *thought* by an accompanying 'I' (*stricto sensu*). The principle of apperception is not a principle of the possession of representations, or even a principle of representation (an interpretative mistake that goes back to Karl Reinhold; see Ameriks 2000). Kant makes this point clear by adding that if representations did not fulfill this requirement, they "would be nothing for me" (B132), and not that they would not exist. Kant does not make an existential claim here; nor does he make a claim about an absolutely necessary entailment between representations and the 'I think'.[35]

A similar situation arises in the footnote. It is insofar as the unicity of space *is to be something for me*, and therefore to contribute to my experience of an objective world, that it requires a synthesis. Put differently, insofar as I *take* the unicity of space *as* a unity of a spatial region, it requires a synthesis. This means that, in the abstract context of transcendental analysis, one could in principle very well consider spatial representations where the unicity of space does not fulfill this requirement, for example, when we consider space as an infinite given magnitude. However, such representations would play no role in cognition other than in these abstract considerations if they could not be brought under TUA through a synthesis. Therefore, although what Kant refers to in the second sentence of the footnote is the unicity of space, this is only to remind us that *insofar as* it is grasped by the understanding, the unicity of space presupposes a synthesis. That is, the unicity of space has a function in discursive knowledge through the grasping of this unicity of space by the understanding as a unity *for* the understanding, which grasping involves a synthesis.

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[35] See further on this Schulting (2012b).

The use of "to be sure" (*zwar*) in Kant's footnote is telling here. This is an expression that typically characterizes a discussion in which someone concedes a point that he recognizes. But with whom is Kant discussing these issues? We would argue that he is agreeing with the reader who has just made sense of the earlier part of TD. Why? Because the key claim of TD's first part is that TUA, which is the highest point to which the possibility of all cognition is to be referred, is a synthetic unity (B133–34) (see again section 2 above). Therefore, if space and its sui generis unity are to play a role in cognition, as Kant claims in the second step of TD, there must be a synthesis of space serving as constraint on the analytic unity of apperception so that perception of objects is made possible.[36] Such a synthesis would, moreover, be a condition of all concepts of space, in line with Kant's claims in the footnote. As a result, the two challenges for a nonconceptualist interpretation we flagged at the outset in this section can be met: we have an understanding of why a synthesis is required, namely, to grasp the unicity of space as a unity; and the role of TUA as highest principle of all knowledge is not threatened by the unicity of space because it can be brought under TUA through this synthesis.

The unicity of space is thus relevant to cognition insofar as it is *taken as a unity*, and this "taking as" is precisely reflected in the requirement of a synthesis. It is this synthesis-requiring unity that defines the notion of *formal intuition* that Kant differentiates, in the first sentence of the footnote, from that of a mere *form of intuition*, which "merely gives the manifold."[37] That is, from the perspective of the understanding—which is indeed the perspective of the footnote—if the spatial manifold that is brought under TUA through a synthesis defines a notion of *formal intuition*, in contrast, without such a synthesis it is the form of a mere manifold of appearances, namely, the *form of intuition.*[38]

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[36] TUA only enables the cognition of an object in general. Insofar as the perception of empirical spatial objects is possible, a further analysis of the conditions of such perceptual knowledge is required. What comes into play here is the synthesis of the imagination, or the figurative synthesis introduced by Kant in sec. 24 of the B-Deduction, which is the synthesis that determines space. This latter synthesis serves as a further constraint on TUA, in order to enable the cognition of spatially located empirical objects. It is also for this reason that Kant divides the B-Deduction into two consecutive argumentative steps, corresponding to the two kinds of cognitive constraint on our representations of objects.

This account of form of intuition and formal intuition disagrees with Longuenesse (1998a, 212–27; 2005, 67–73), who has argued that the pure intuition of space of TAe, which is the form of intuition, and formal intuition are not really distinct; that is to say, she claims that space as pure intuition just *is* a formal intuition. The reason for Longuenesse's conflation of the two notions can be found in her claim, which we discussed above, that the synthesis of the imagination (*synthesis speciosa*) is in fact responsible for the *production* of space (and time) in its very givenness (2005, 76–77). This claim leads Longuenesse to read TAe as already describing the formal intuition of space, a claim that is hard to square with Kant's explicitly presenting TAe as dealing with the "pure form of sensible intuitions" (B34/A20; see also Allison 1983, 97). Moreover, Longuenesse's conflation of form of intuition and formal intuition collapses the distinction Kant explicitly makes between the merely given manifold (which at any rate has the necessary form of intuition) and the determinate manifold (formal intuition), which Kant is keen to bring out in the footnote.[39] And why would Kant make a point of distinguishing between the "form of intuition" and "formal intuition" if they were not really distinct, as Longuenesse suggests? Longuenesse is indeed aware of the need to provide some account of Kant's distinction. Her solution, however, which involves appealing to the epigenesis of space, is problematic (Longuenesse 1998a, 222; 2005, 28–29; 2000, 104), since her claim that "form of space" refers to the unknown ground of the formal intuitions of space, where "ground" refers to the genesis of such intuitions, has no firm basis in CPR.[40]

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[37] The notion of "form of intuition" we are using here is incompatible with interpretations of it as involving rules, since we understand rules to be involved only at the level of the synthesis of space. Thus, Peter Krausser (1973, 282) claims that the form of intuition is to be understood as a set of rules, and therefore as the form of intuit*ing* rather than of the intuit*ed*. This latter distinction was examined by Patricia Kitcher (1990, 37) as one between "process form" and "product form." The former notion is proposed by Kitcher to answer the question of how sensations are ordered spatially and how an intuitive representation in space is constituted. Krausser does not view the rules defining the "process form" as a kind of spatial schematism but wants these rules to be distinct from the rules defined by the categories and implemented through the transcendental synthesis of the imagination. Rather, the latter rules operate "in conjunction" with the first to produce formal intuitions. Such an interpretation leaves it unclear how these rules stand with respect to TUA. Furthermore, it overlooks the important phenomenological dimension of the form of space, that *wherein* the manifold is represented.

[38] To mark the contrast with the notion of "formal intuition," Melnick (1973, 11) refers to space as a "pre-intuition."

[39] Konstantin Pollok (2008, 333–34) appears to make a similar conflation. He writes: "The synthesis of apperception is required not only for providing the manifoldness with a *conceptual* form, but also for the specifically *spatiotemporal* form it happens to possess. This manifoldness has a *material* aspect, the brute sensation by which the subject is

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affected and a *formal* aspect, the 'transcendental synthesis of the *imagination*, which is an effect of the understanding on sensibility' (B152), by which the sensation obtains its spatio-temporal structure in the first place." Also, Pollok (2008, 334n25) makes the puzzling claim that "the purely 'given' elements of experience" are "mere effects on our sensibility (brute sensations) that are not even possessed of spatio-temporal form." This is puzzling insofar as any sensation must at least have the form of time to be able to be "received" at all. It appears that Pollok, like Longuenesse (2005, 72), sees the distinction between receptivity/givenness and spontaneity/synthesis as mapping onto the distinction between matter and form, so that any form (form of intuition and formal intuition) would be the result of the synthesis of the understanding (hence, Longuenesse's conflation of form of intuition and formal intuition), and the givenness of the underlying matter or material aspect of experience would just be a brute fact or some amorphous raw material. Two observations are in order: (1) it is not just that the effect of the understanding is that it determines any given manifold by giving it some form, also the matter of what is given is determined qua matter (under the categories of quality); (2) one should be careful not to conflate the empirical and transcendental levels of analysis: readings à la Longuenesse and Pollok run the risk of committing Kant to some phenomenalist form of idealism, whereby the objects of outer sense are constructed out of mere sensations.

[40] Longuenesse draws upon Kant's response to Johann Eberhard in Disc as evidence for her claim. There are however no grounds for thinking that what Kant refers to as the "first formal ground" and the "ground of the possibility of sensory intuition" (Disc, AA 8:222 [Kant 2002, 312]) is what CPR refers to as "form of intuition." The fact that it is innate, moreover, suggests that it cannot be the form of space as a priori representation (the spatial manifold that Kant alludes to in the footnote). The text of Disc, AA 8:222, is more problematic in its use of the terminology "formal intuition," which does not, on our interpretation, refer to the same notion of "formal intuition" as in CPR. Indeed, in his response to Eberhard, Kant expressly contrasts formal intuition to empirical concepts, which depend upon the categories. We would therefore contend that Kant is here using "formal intuition" loosely (as he does in CPR, A268), and would back up that view with the following three observations. (1) The context is one of a discussion of the acquisition of a priori representations in response to Eberhard's Leibnizian views. In this context, the issue is not whether a representation of space functions as a unity or not, that is, it is not the issue at stake in the footnote. Rather, the point that Kant is making is that this intuition is an "originally acquired representation" (Disc, AA 8:222 [Kant 2002, 313]). By contrast, in the footnote, a formal intuition is described as containing a unity that "requires a synthesis." (2) Kant refers in the same sentence to this formal intuition as a "form," and similarly on the previous page (Disc, AA 8:221). Finally, (3) we note the use of the qual-

That there is a transcendental role (that is, not a mere epigenetic one) for a notion of space as mere form of intuition that is *independent of the understanding or even the imagination* is confirmed by Kant's referring to the "*synopsis* of the manifold *a priori* through sense" in the Transition to the transcendental deduction of the categories (A94/B127). Heidegger makes this notion of synopsis into a cornerstone of his interpretation of Kant's notion of space and correctly connects it with the notion of form of intuition as Kant presented it in TAe (A94–95/B127).[41] However, he then wrongly attributes it to the activity of the imagination (Heidegger 1991, 141–44).[42] In fact, the synopsis is the receptive gathering of sensi-

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ification "long" in Kant's claim that this formal intuition "long precedes the determinate *concepts* of things" (Disc, AA 8:222 [Kant 2002, 313]). This certainly leaves room for the suggestion that the inclusion of the word "long" introduces a significant difference to Kant's claim in the footnote that the unity of formal intuition "precedes all concepts." The distinction would precisely lie in the fact that if the form of intuition is at stake, this must be brought under TUA (thus defining the formal intuition of space in the sense of CPR) to function as background for objective empirical determinations. That this is not spelled out by Kant lies in the fact that the determination of the form as formal intuition is always to be understood in terms of the determination of possible empirical or sensible manifolds in space of which it is a condition. Longuenesse's appeal to the epigenesis of the form of space is an interpretative move that parallels her understanding of a prediscursive role for the understanding. In both cases, she is making use of Kant's gestures toward genetic grounds of our a priori intuitions, on the one hand, and toward a possible common root of the understanding and sensibility, on the other. Just as much as her understanding of the notion of form of space makes use of the first ground, her notion of a prediscursive role for the understanding that generates the forms of space and time goes a long way toward addressing the second issue. In that sense, Longuenesse's interpretative stance is informed by a concern for the grounds of Kant's system that is not dissimilar to that of the German Idealists. In agreement with Fichant (1998, 92), we view Longuenesse's introduction of the notion of a prediscursive role for the understanding as one of the most problematic aspects of her interpretation.

[41] See Banham (2006, 10–12) for discussion.

[42] Heidegger does however argue for a synthetic unity, one of a sort that is not due to the imagination, let alone the understanding, in an illuminating section of his lectures on Kant's CPR (Heidegger 1995) devoted to our footnote. There, Heidegger refers to space and time as wholes that have their proper unity (*Einigkeit*), a unity that is not "an additional [*nachträgliches*] product of a subsequent unification" (1995, 134; our translation). Heidegger (1995, 135) refers to "syndosis" as a better term than "synopsis," which still suggests that I "intuit the manifold together one after another [*hintereinander zusammenschaue*]" (our translation). Syndosis, by contrast, signifies the already-togetherness of the given manifolds of space and time: "this unity of the syndosis is not the unity, which belongs to the synthesis of the understanding in concepts, that is, to the categories. Rather, this synthetic unity of concepts, of categories, presupposes the former originally intuiting syndotical unity" (Heidegger 1995, 135; our translation). We agree with Heideg-

ble manifolds as aggregates of representations within the spatial form of intuition whereby they are not yet represented *as* anything. It is only through a *synthesis* that sensible manifolds are represented *as* manifolds of representations of an object, and that space is determined as formal intuition. If the distinction between form of intuition and formal intuition appears at this stage in Kant's analysis, it is because it only makes sense from the point of view of the understanding. But this must also mean that TAe can now, *retrospectively*, be viewed as dealing with space both as form of intuition and as formal intuition.

In this sense, Longuenesse (2005, 34) is right to say, contra Allison's reading of her interpretation, that she does not intend to see section 26 as a "revision," but rather as a "re-reading" of TAe: "Everything that was said in the Transcendental Aesthetic about the nature of space and time stands, but it is brought into new light by the argument of the Deduction. . . . Space and time, then, are forms of sensibility, just as Kant maintained in the Transcendental Aesthetic. But they are forms of a sensibility affected by the understanding, and thus they are the product of *synthesis speciosa*, the transcendental synthesis of imagination." We agree that nothing of what was said in TAe is retracted by Kant in section 26 of TD (contra Dörflinger 2010, 71 and 75–76n3). The point on which we however fundamentally disagree with Longuenesse concerns the claim in the latter clause in the above quotation, namely, her claim that the *forms of sensibility themselves*, rather than their *unity*, are the product of the imagination. We distinguish between the mere form in which manifold sensations of outer (or merely inner) sense are *given* and the way they are taken *as* unified manifolds, which constitutes space (and time) as formal intuitions. Importantly, this distinction is not just a distinction between the matter of the manifold and the form of a pure intuition independently of this matter (which in a way would be endorsed by Longuenesse; compare Longuenesse 2005, 71–72), but is a qualitative distinction between the unicity of space as a given infinite magnitude and space *taken as* a unified intuition, only the latter of which requires the fulfillment of the condition of an act of synthesis.

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ger's account insofar as he does not speak of the imagination's role. Heidegger is right that the givenness is a necessary condition for the possibility of the function of combination carried out by TUA; combination requires combinability in the given. We do not agree, however, with Heidegger's general implication that the syndotic unity of the manifold would have a *grounding* role in this respect.

5. The Concept of Space

Having presented the key claim of our nonconceptualist interpretation, we need to clarify what characterizes space as a nonconceptual unity. To do this amounts to forming a concept of space. In the titles of ME in the B-edition of CPR, Kant's use of the terminology "concept of space" is prima facie puzzling.[43] Insofar as Kant is precisely going to claim that "space is not a discursive, or as is said, general concept of relations of things in general" (A24/B39), this would seem tantamount to a contradiction, for in this statement, Kant is referring to the representation of space and claiming that it is not a concept, so how can he also talk about the "concept of space"? The usual way of dealing with this problem is to treat it as a sloppy piece of writing, that is, as standing for "representation" (compare Fichant 2004, 534n8). In support of this view, one might well point to the somewhat sloppy use of certain terms that one occasionally finds in CPR (for example, the use of "appearance"). But often one finds that there are distinct specific meanings attached to these terms within the context in which they appear, which could explain why such terms appear in two different guises in the same passage. Aside from that, it is worth noting that Kant has already referred to "representation" as a general term including both concepts and intuitions in the earlier part of TAe (A20/B34),[44] so it is not clear why he would revert to "concept" to mean "representation."

And indeed, when we look more carefully at the use of "concept," it is noticeable that when Kant says that "space is not a discursive ... concept" (A24/B39), it is space itself that is at stake, not its representation. What Kant has in mind here is a view of space as consisting of relational properties, as Leibniz would have it. Whatever the nature of space might be, it is always possible to form a concept of it, and this is what ME discusses. However, forming a concept of space does not imply that space is thereby fully captured conceptually, a point that the Dufour/Cohen interpretation overlooks. By rejecting this, we are saying that the concept in question is rather a description of the content of the representation of space, that is, of *spatiality* (spatialness). And since the content in question is a priori, such a concept is acquired in transcendental reflection.[45] In TAe, characterizations of the intuition of space, *qua form of intuition*, are presented. These represent marks of a concept of space *qua form.*

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[43] Brandt (1998, 91) notes that "concept" appears more prominently in the B-edition. It is nevertheless the case that the term is also used in the A-edition at least once (A26/B42; heading), even though it is arguably unclear whether this use refers to the concept of space or the outcomes of the analysis of the representation of space in the metaphysical and transcendental expositions.

[44] See the *Stufenleiter* at A320/B376, where Kant points out that while "representation" is the genus, concept and intuition are species of "representation."

What are what Kant would call the "marks" (that is, the characteristics arising from an analysis) of the concept of space? They are the apriority (A23–24/B38–39) and intuitive nature (A24–25/B39–40) of space. The latter, more specifically, is characterized in terms that are the opposite of those of conceptual representations. As analyzed above (section 3.B), the characteristics of singularity, infinity, and mereological inversion identified in TAe constitute the unicity of space, which is what the unity of space amounts to prior to any involvement of the understanding. But, in addition to the unicity of space, four properties of space characterize it as a priori intuition:

(1) There is a property that Kant only mentions in the Anticipations of Perception. This is the property of *continuity.*[46] Understood by Kant in terms of flowing quantities and viscosity (Posy 2008, 185–86),[47] continuity is a topological feature characterizing the fullness of space: there are no gaps in

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[45] This comports with the regulative function of transcendental enquiry. Here, the analogy with Carl Posy's distinction between *modeling* scientific enquiry and the scientific enquiry itself is useful (Posy 2008, 180–81). At the level of transcendental philosophy, when we reflect on "intuition," we abstract from its cognitive role as "receptivity" (Posy 2008, 180). Some commentators have pointed out the difficulties that Kant is seemingly confronted with in trying to describe the properties of space, among which is the key property that it is not conceptual (Fichant 1997, 23), for we could say that space is nonconceptual only by employing concepts.

[46] Kant mistakenly takes continuity to follow from the infinite divisibility of space (A169–70/B211–12; A527/B555). As Friedman (1992, 60ff.) points out, he thereby does not differentiate the topological property of *everywhere denseness* (which follows from the infinite divisibility of space) from that of *continuity.* Indeed, it was not until Dedekind (1963) that the notion of continuity was given a clear formal definition. Vuillemin (1967, 333) notes the absence of a mention of this property in TAe, but interprets this as implying that it is a nonintuitive feature of space.

[47] Friedman (1992, 78) claims that this Eulerian conception of continuity defined in terms of flow is not generally sufficient to allow for continuous curves that are nondifferentiable at an infinite number of points. Examples of such curves are fractal objects such as the Koch curve. It is not clear that this is a real shortcoming though, as such curves are obtained as limits of curves that are differentiable except at a finite number of points. The issue then reduces to that of Kant's notion of infinity and the extent to which he can allow for such infinite objects (on this, see Posy 2008).

it, that is, "one can never represent that there is no space" (B38/A24). In what follows, we shall add the property of continuity to the defining characteristics of the unicity of space.

(2) Next, space is characterized as enabling the representation of one thing as outside another (A23/B38; in the above-quoted *Duisburg Nachlaß* fragment, Kant specifies as one of the characteristics of space that it is also a "means for ordering [outer things]"; AA 17:641 [Kant 2005, 157]). In particular, this feature enables the representation of mere numerical difference, as Kant points out in the Amphiboly (A263–64/B319–20). This property defines the way in which the parts of space are related to one another. It amounts to a further characterization of the topology of space,[48] which we shall refer to as *externality*.[49]

(3) The construction of geometric objects (B39/A25) does not depend upon where or how they are located in space. This requires that spatial properties should not be dependent

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[48] Friedman (1992, 60ff.) makes much of the fact that such topological features as *everywhere denseness* and *continuity* can be represented in polyadic logic. That a more advanced logic than the one Kant used should enable the formalization of certain features of spatial intuitions does not, however, imply that spatial intuition can be replaced with conceptual representations, since the phenomenological aspect of space is not captured by such a logic. It is possible to understand the logical formula without having any understanding of the spatial property it represents; what is missing though in such a logical formula is the phenomenological content that requires having a spatial intuition. There is therefore a sense in which Luitzen Brouwer (2005) is correct in pointing out that topology is not formalizable. Brouwer (2005, 93n57, 103n63, 132–33) contrasts mathematical "primordial intuition" with what he describes as a "mathematical system of the second order," namely, mathematical "constructive possibilities," that is, all mathematical judgments; in other words, the primordial intuition is irreducible to second-order logical description. A related issue is that the formalizations of many topological and geometric notions do not define concepts in the Kantian sense. That is, they do not provide a set of marks defining a concept under which a possible object could be brought.

[49] In a topological space, this feature is captured formally by the notion of *normality*. Rudolf Carnap (1922, 23) claims that the topological features of what he calls "intuitive space" are the only ones to be intuited. According to Thomas Mormann (2008, 46), one should understand these as "local topological properties of space." While this "local" nature of topological properties is arguably also a feature of the notions of exteriority examined by Kant, the further Carnapian claim that anything nonlocal (which, for Carnap, will be the geometric properties of space) is a matter of convention overlooks the fact that the construction of geometric figures requires that an infinite space be represented in intuition.

upon spatial location, that is, that space is *homogeneous.* The geometrical properties of invariance under translation and rotation follows from this.

(4) Finally, Kant characterizes space as enabling the representation of what is outside me (A23/B38), which we refer to as the property of *centered externality*. Together with the uniqueness of space, this means that, as noted above (section 3.E), space defines outer sense. It is the form of all possible reality of outer sense.[50]

These additional "topological" features of *continuity, externality, homogeneity*, and *centered externality* contribute to a fairly well-defined concept of space acquired through transcendental reflection,[51] which amounts to a proper phenomenological investigation into the nature of space (Fichant 2004, 532 and 550).[52]

One might then ask why geometry is mentioned in ME. This is to bring out how the characteristics of space as a unicity, in particular the fact that it is single and that all spaces are parts of it, explain how it grounds the possibility of all spatial representations. Kant focuses upon geometry because the focus of CPR is synthetic a priori knowledge, but the claims about synthetic a priori knowledge that are made in CPR also have implications for the synthetic a posteriori knowledge of empirical objects of

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50. To call this property of centered externality "topological" requires a broad notion of what "topological" means. We use this term insofar as what is at stake is the property of X being outside Y, even though in this case only one of the terms is an object and the other a subject. This is the notion of "outside" involved in the very definition of "outer sense." Note that this property has a phenomenological feature, namely, one that relates directly to my embodiment. The possibility of representing something as outside me is key to my being able to orient myself as an embodied creature (see DDS, AA 2:377–83). Here, importantly, Kant refers to the "feeling" (*Gefühl*) of orientation, which is an aspect of Kant's somewhat covert awareness of the issue of the embodied self (compare Onof 2010).

51. The term "topological" is here used in a broad sense rather than to refer to the mathematical discipline of topology: topological features are those pertaining to how the parts of something are arranged. See also the previous note.

52. Fichant (2004, 532) points out that Kant could be taken to task by a phenomenologist for appearing to steer his whole discussion of space around the identification of transcendental features. Edmund Husserl, on the contrary, claims that the eidetic reduction must precede the transcendental one, while the Munich school of phenomenology distanced itself from Husserl's transcendental turn (Moran 2000, 77). Nevertheless, we agree with Jocelyn Benoist's (2004, 528) analysis of the nature of Kant's investigation, namely, that Kant proposes a "doctrine of the appearing (or the 'phenomenon')," which clearly amounts to a "phenomenological" investigation. See also Parsons (1992) and Carson (1997).

outer sense. So the characteristics of space define the realm of possible spatial determinations: in any determinate spatial intuition, there is an intuition of space as the locus of these determinations and this is the formal intuition of space.

More specifically, we can see how each property of space as form of intuition that we identified above defines a determination of space in its function as background to all objective spatial representations:

(a) The unicity of space (in particular its singularity and mereological inversion) defines space as the unique horizon for all possible objects of outer sense and all geometric constructions, since all spaces occupied by such objects must be parts of this single space. The infinity of space as form of outer sense is required so that the determination of space can enable the representation of any object however far and however large, and any geometric construction however extended it may be (see again AA 17:641). As Kant shows in the Axioms of Intuition (B203), the representation of "the homogeneous manifold in intuition in general, insofar as through it the representation of an object first becomes possible, is the concept of a magnitude (*Quanti*)." The *quantitas* (quantity) of such a magnitude (quantum) is determined through the synthesis of composition (that is, one of aggregation; B201n), which can be completed only for finite magnitudes. It is therefore not possible to cognize an infinite object. What conceptual thought can do, however, is to consider "the synthesis of a series insofar as it is never complete" (A510/B538), that is, to consider the concept of a potential infinite defined through the notion of *progressus in indefinitum* (A511/B539). Thus, in his response to Kästner, Kant differentiates the potential infinity of space, which is required by the geometer as ground for his or her determinations of space, from the actual infinity of space that is presupposed as subjectively and originally given (see further Onof and Schulting 2014).

(b) But equally, through the infinite divisibility implied by the mereological structure of space, the determination of space enables the representation of any object however fine grained or small it may be. Insofar as the consideration of such limitlessly small parts of space is the consideration of the infinitely small, conceptual thought is again confronted with a notion of infinity in intuition.

(c) The property of continuity of space means that we cannot represent anything like the absence of space (B38/A24). That there are no gaps means that space is necessary for any representation in outer sense. Discursive thought can grasp the continuous texture of space by a type of synthesis whose generation is "a progress in time" (A170/B211), so that this grasp is dependent upon the grasp of time as continuous.[53] This seems to point to the issue of dealing with how the continuity of time is brought under TUA. But here we encounter the problem that it is not possible to represent time without drawing a line in space, as Kant asserts frequently (for example, B154). So any determination of the continuity of time refers back to that of space. What gets us out of a potential vicious circle here is the observation that this notion of continuity is discussed in the Anticipations of Perception (A167–70/B209–12). Here, we see the continuity of time as the condition for the representation of the continuous variation of the "intensive magnitude" of a possible sensation. The continuity of time can thus be grasped (indirectly) as the transcendental condition of the continuous variation experienced in our sensory inputs. The fine-grainedness of this variation is not limited: it is a potentially infinitely fine resolution, as defined by a regress *in infinitum* (compare A510–14/B538–42). We thus get an underdetermination here that mirrors that of the potential infinite of the extensive magnitude of space.[54]

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[53] Compare Posy (2008, 184–85). Posy points out that Kant took over Leonhard Euler's visual depiction of motion. The continuity of space is thus intimately bound up with the possibility of the productive synthesis. See, by way of comparison, Kant's description of the productive synthesis of imagination in terms of motion as an action of the subject at B155 and B155n.

[54] The apparent complexity of this determination of space through the categories of quality is not surprising: unlike the case of the infinite magnitude of space, we are dealing with the infinitely fine structure of some possible empirical intuition. This cannot be grasped directly through a synthesis of composition, even if reiterated indefinitely. Rather, this infinitely fine structure is grasped by referring to a possible empirical synthesis.

(d) The topological property of spatial externality implies that the determination of space must enable the (simultaneous) representation of any plurality of objects whose only difference is numerical. To enable this, discursive thought can formulate the relational concept of mutual exteriority of the components of a partition of space.[55]

(e) An additional requirement for geometry is that there be no location specificity to constructions in space: when I construct a triangle, there is no reference to where it is constructed, which means that space must be *homogeneous* with respect to the basic transformations of geometry, which are translation and rotation.[56]

(f) An additional requirement for outer sense is that it be possible to represent objects outside me: this is ensured through the property of *centered externality*.

That we are able to form such a rich concept of space is evidence of an intimate phenomenological acquaintance with space. But it also explains why it is possible to determine space under the twelve categories. That is, the knowledge of spatiality that is contained in the concept of space is the key to the possibility of bringing the unicity of space under TUA, thereby determining space *as* the form of outer sense. This follows from the observations above as to how the characteristics of the unicity of space each define determinations of space as background to all determinations of objects in outer sense.

The phenomenological acquaintance with space that enables this is therefore comparable to the "hidden art in the depths of the human soul" (B180/A141) in the case of time that underpins the schematism of our understanding. What Kant is drawing our attention to are the limits of transcendental investigation when it comes to identifying grounds for our ability to bring time under TUA. What our notion of phenomenal acquaintance points to is the phenomenal dimension of our ability to bring the unicity of space under TUA.[57] We examine the result of bringing the unicity of space under TUA, that is, the a priori determinations of space under the twelve categories, in the next section.

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[55] Such a formal characterization only amounts to bringing a topological structure under a concept: it does not in any way exhaust the nature of this structure. See note 48 above about the formalization of topology.

[56] Interestingly, this notion of homogeneity as involving qualitatively identical parts, and whose role in mathematics is discussed by Sutherland (2004), is replaced by a more "conceptual" interpretation in Longuenesse (1998a). We understand this as consonant with Longuenesse's neglect of the nature of (spatial) intuition *independently of* the understanding.

6. How Is the Unity of Space Determined under the Categories?

Kant would appear to have more to say about the determination of the unity of time than for space. In the chapter on Schematism, the way in which each category is able to produce a corresponding type of unity of time is described in terms of schemata, which are defined as "transcendental time-determination[s]" (A139/B178).[58] As for space, although in the Schematism chapter schemata of "pure sensible concepts" in space (geometrical concepts) are briefly mentioned as "rule[s] of the synthesis of the imagination with regard to pure shapes in space" (B180/A141), this does not amount to categorial determinations definitive of the unity of space as a whole. What this does bring out is that the categorial determination of regions of space, unlike that of the intervals of time, is mediated by pure sensible concepts.[59]

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[57] An interesting new interpretation of the footnote by James Messina (2014) has come to our attention shortly prior to the publication of this article. There is no space to examine his interpretation in detail, but we note his concern with the "brute given" (Messina 2014, 8) arguably characterizing nonconceptualist interpretations such as our own. Messina (2014, 19) claims that if space were such a "brute given," there would be a "contingent" "fit between the unity of space as our form of intuition and the OSUA [Original-Synthetic Unity of Apperception]." For Messina, such contingency would have consequences for the very possibility of self-consciousness. This contingency and the implications for the possibility of self-consciousness concern however *conceptual* modalities, as Messina himself notes when he considers nonhuman beings with the same kind of understanding but different forms of intuition. But Kant does not claim a conceptual necessity for his a priori conditions of discursive knowledge, for the a priori defines only what is *necessary for human beings* (compare Gardner 1999, 122). As a result, it is not relevant for the truth of the analysis of the a priori conditions of human discursive knowledge that there could be "non-human discursive beings whose forms of intuition lack unity" (Messina 2014, 19). While Messina (2014, 34) seeks further grounds for rejecting the notion of the "brute given" in the development of the pre-Critical Kant's understanding of space, his analysis of Kant's pre-Critical writings serves mainly to connect the unity of space with an intellectual intuition. Messina then suggests that there is evidence that Kant replaces this divine intuition with the discursive understanding. But his attempt to fend off the criticism that Kant changed rather to an understanding of the unity of space as defining an "analytic truth" requiring no further ground (as the nonconceptualist would claim) (Messina 2014, 38–39n56) is not convincing.

[58] On the issue of the role of determinations of space in the Schematism chapter of CPR, see Onof (2008).

The previous section has shown how the determination of space as formal intuition is a determination of it as a necessary condition of all spatial determinations, whether geometric or empirical. Examining the conditions for such spatial determinations, we found that they corresponded to an underdetermination of space as a whole (through notions of potential infinity). We can now examine more carefully, albeit briefly, how each of the twelve categories of quantity (unity, plurality, totality), quality (reality, negation, limitation), relation (inherence and subsistence, causality and dependence, community) and modality (possibility, existence, necessity) is involved in determining space:

(1) In terms of *quantity*, space is the unique background that acts as horizon for the representation of all objects. This universality means that space is thus determined under the category of *unity.* Further, it is always possible to carry out a partition of space (for example, what is on my left and what is on my right), so that space is also determined under the category of *plurality*. Space, which has an infinite magnitude, cannot simply be brought under the category of *totality* in terms of a discursive determination, which is always finite. But, as we saw above, Kant distinguishes between actual and potential infinities. If we thus consider a series of spaces, for example, with each term of the series double the size of the previous term in all directions, we obtain a progress *in infinitum* (A510/B538). As in the case of the division of matter to infinity, "the further members . . . are themselves . . . given prior to this" series (A513/B541). This defines how space is determined under the category of *totality*.

(2) In terms of *quality*, all sensation in an empirical intuition has a particular degree of intensity, which corresponds to a reality in outer sense (B209/A168; compare B182/A143). Since space is the form of outer sense, it is thereby determined under the category of *reality* as the form of all that is real in outer sense: "perception thus represents . . . something real

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[59] This is in line with the idea that space is the form of outer sense, so that empirical objects or, more precisely, sensations as referring to such objects are always involved (either directly or indirectly) (see Butts 1981).

in space" and "only what is represented in it can count as real" (A374). Space is however not itself any thing: as "pure space," it is an "*ens imaginarium*" (B347/A291). In other words, space as "pure (merely formal) intuition" (B207) does not itself contain sensations (B44/A29), and hence *pure* space is not an intensive magnitude by means of which it would correspond to a real object or to an absolute reality (A37/B54). It is thus as "empty intuition without an object" (B348/A292) that it is determined under the category of *negation,* where negation corresponds to zero degree intensity and to the absence of reality (B208, B209–10/A168) or to "nothing" (B347/A291).[60] Since space itself is no intensive magnitude, it can be determined only through the intensive magnitude of that which fills it, that is, what corresponds to sensation in outer sense, namely, matter. In the chapter on Dynamics in the *Metaphysical Foundations of Natural Science,* Kant examines the filling of space by matter. This is the spatial equivalent of the filling of time by space, which is the key to the determination of time under the categories of quality (B182/A143). Kant thus shows that "matter fills its space through the repulsive forces of all of its parts, that is, through an expansive force of its own, having a determinate degree, such that smaller or larger degrees can be thought to infinity" (AA 4:499; Kant 2002, 211). By considering an endless increase of this degree, we define a progression *in infinitum,* through which space as a whole is determined (as that which is thus filled) under the category of *limitation.*

(3) In terms of *relation,* space is determined under the category of *inherence and subsistence* as the homogeneous "container" to which all parts of space refer: this is a consequence of the mereological inversion property through which every determinate space refers to the background space of which it is a part. This then raises the question: how is a particular region of space determined? As Kant repeatedly emphasizes (for example, B154), it is through construction in intuition, that is, in this container space, that a particular region of space is determined. What he thereby underlines is the

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[60] "Nothing" here means "a concept of the absence of an object," and so points to empty space.

role played by the intuition of space in providing the ground for any particular determination of space. That is, any particular region of space is determined as dependent, or grounded, on the whole of space, in which it necessarily inheres. In this way, the relation of the whole of space to a determinate space is determined under the category of *causality and dependence.* Further, any division of space defines a partition of it, which provides a determination of the regions therein under the category of *community*: any particular region of space is in *community* with the rest of space, or put differently, all regions of space are simultaneous (see, by way of comparison, B256–57).[61] To determine the whole of space under the category of community, it is necessary, as in the case of the category of totality, to consider a progression defined by a series of such determinations. Here, each step of the progression divides space into two regions in community, whereby each of the regions conditions the other. This division can be characterized either as a right-left or a foreground-background division. By making the one region (for example, foreground) smaller at each step of the progression, space (for example, background) as a whole is determined through a progression *in infinitum.*[62]

(4) In terms of *modality,* recall that space is the condition of possibility of all perceptions of appearances in outer sense. To think space under the category of *possibility/impossibility* is to think of it as the enabling ground of all outer sense. As such, space is determined under this category. Space is not determined as itself an existing object (A429n/B457n). However, as the ground of all that exists in outer sense, it is determined under the category of *existence* or *actuality.* If space is thus defined as a necessary condition of the actuality of objects in outer sense, space is determined under the category of *necessity* in a conditional sense.

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61. It is important to note this independence of time: the categories of relation are not applied here as dynamical categories because space is considered in its independence from time.

62. Note that, as for the category of totality, the progression is *in infinitum* because the whole of space is given prior to it (A510–13/B538–41).

In the above outline of how space is determined under the categories, notice that the determination involves an inevitable appeal to potential infinities in the case of the third categories of the groups of categories of quantity, quality, and relation. This is a feature of space insofar as it provides a *horizon* for objects of outer sense.[63]

7. The Synthesis Grounding the Unity of Space

Having now given an interpretation of how the unicity of space is grasped as a unity by means of TUA under the categories, the question arises as to the nature of the synthesis that is involved in bringing space under the categories. We need to find a way of interpreting the synthesis grounding the unity of space in such a way that it involves an application of the categories, so that all concepts of space (that is, concepts of determinate regions of space in outer sense or geometric concepts) are thereby conditioned by TUA and that through this synthesis all concepts of space and time first become possible, as Kant indeed claims in the footnote. The footnote is appended to the section where Kant examines the synthesis of apprehension and claims that its unity must conform to the categories (B161). But this synthesis operates upon empirical rather than a priori representations: it is that by which the manifold of representations in an empirical intuition is brought together as a manifold of representations insofar as an empirical object is represented by means of it (B160). What does this mean? Apprehension is the function through which any manifold is taken as referring to an as-yet indeterminate object: the mere givenness of the manifold involves only the receptivity of sensibility, but its being taken to refer to an object, by means of the synthesis of apprehension, is the first logical step in Kant's analysis of the constitution of objectivity. This step is more clearly separated out in the A-version of TD, where it logically precedes the reproduction and the recognition of the manifold under a concept, through which it is taken as the manifold of a *determinate* object.

What can be said of the role of space in apprehension? Every representation of outer sense is given in space as form of intuition. The unicity of space ensures that it is the same space for all these representations. However, *pace* Friedman (2012, 243–44),[64] this is not sufficient to explain why the manifold of representations is taken as referring to the same space. The important move that we identified earlier as explaining the apparent obscurity of the second sentence of the footnote is now this: the unicity of space must be *taken* as a unity, which means that I must, as cognizing subject, *take* the space of all these representations of outer sense as one. In the pure receptivity of the "mere manifold" (space as "mere form of intuition") (B160n; translation amended), there is no conceptual grasp that the space in question is unitary. The conceptual grasp of space as unitary involves a separate role for the spontaneity of the understanding.

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[63] More specifically, the infinite magnitude, infinite divisibility, and mereological inversion of space account for these potential infinities, while the status of the *conditional* necessity of space for the possibility of empirical reality is how space is defined under the third category of modality.

Evidence of Kant's awareness of this issue is mainly found in the A-version of TD where Kant says, "Now *this* synthesis of apprehension must also be exercised *a priori*" (A99; emphasis added). Kant presents this pure synthesis as an a priori version of the synthesis of apprehension of empirical manifolds and as serving to unify a priori manifolds (space and time). The *transcendental* function of this synthesis is, moreover, stated clearly: without it, we would have no a priori representations of space and time (A99–100), as the representations of space and time would be nothing *for me* without this synthesis.[65] All this is in line with the footnote: the formal intuitions of space and time are made possible by this pure synthesis. It therefore seems apposite to interpret the synthesis referred to in the footnote as this pure synthesis of apprehension.

In terms of the interpretative requirements we set out at the beginning (section 1), a worry may arise insofar as it is not obvious in what sense it can be said that "through [synthesis] all concepts of space . . . first become possible" (B160n). And indeed, the B-version of TD assigns a key role to the imagination that was already the focus of the A-version, namely, the transcendental synthesis of the imagination, also called "figurative synthesis," or *synthesis speciosa,* which determines "sensibility a priori" (B151–52) and makes the application of concepts to intuition possible (B151). It might seem as if the synthesis that the footnote mentions is not the synthesis of apprehension, as we argued above, but rather the synthesis of the imagination.

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[64] Friedman is led to this view by identifying metaphysical space (as form of space) with perspectival space, that is, by accounting for it in terms of the totality of possible subjective perspectives. This means that he has to appeal to TUA to bring all possible perspectives together. What he does not notice, however, is that the synthesis of all possible perspectives itself requires a background space and it is this space that as the form of intuition, that is, metaphysical space, is originally and subjectively given (Kant 2014, 309). Since his interpretation of this form involves TUA and thus a grasp of space as a unity, Friedman does not distinguish between having all spatial representations in the single given space and the grasp of this space as a unity for the understanding.

[65] Kant adds (at A99–100) that the manifolds of space and time are contributed by sensibility. This confirms our reading against Longuenesse's, as it supports the view that space and time originate in sensibility *prior to* any synthesis.

In fact, this does not conflict with our focus upon the pure synthesis of apprehension. For Kant clearly states that the imagination is the faculty "whose action exercised . . . upon perceptions I call apprehension" (A120). The synthesis of apprehension is the synthesis of the imagination in the domain of perceptions. As explained above, this involves *taking* the unicity of space *as unity,* thereby determining the formal intuition of space. Furthermore, we note that Kant no longer mentions the pure synthesis of apprehension in the A-Deduction after presenting it in A102, or at all in the B-Deduction, apart from a significant passage at B164. Here, Kant is going over the results of TD. And after reminding us that "that which connects the manifold of sensible intuition is imagination, which depends on the understanding for the unity of [the] intellectual synthesis [of sensible intuition] and on sensibility for the manifoldness of apprehension," he claims in the next sentence that "all possible perception depends on the synthesis of apprehension" and takes this to show that "all possible perceptions, . . . as far as their combination is concerned, stand under the categories" (B164–65). This entailment is possible only if the previous sentence's claims about the imagination are relevant here, that is, if the imagination is indeed what is at work in the pure synthesis of apprehension.[66]

8. Space "as Object" and the Unity of the Formal Intuition of Space as Belonging to Space

Having fleshed out our nonconceptual interpretation of the issues arising from problem 1, we now return to the second and third problems flagged at the outset (section 1). These concern the questions how it is that Kant considers space "as object" (B160n) (problem 2), and why it is that the unity of the formal intuition of space *belongs* to space and is not a conceptual unity coming from the understanding (problem 3).

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[66] For more on the relation between the synthesis of apprehension and the synthesis of the imagination, see Schulting (forthcoming b).

Starting with the first issue (problem 2), we have seen that space is determined under the categories by means of infinite progressions in the case of the third categories in each group of categories. That is, space as a whole cannot be grasped as a totality, through limitation, or in community, in a single determinate intuition: this is a consequence of the properties that we have denoted as the unicity of space. The synthesis of space does not, therefore, give rise to a concept of space that determines it as an object.[67] But the categorial determination of space, however incomplete, does enable the form of intuition to be grasped as a unity under TUA, thereby defining the *formal intuition* of space (in contrast to the mere form of intuition). Since TUA is "that unity through which all of the manifold given in an intuition is united in a concept of the object" (B139), and given that TUA determines the unity of space, it is possible to talk of space "as object" (B160n). Importantly, however, unlike spatiotemporal objects of experience, this does not mean that space itself *is* an object. Rather, since the intuition of space is grasped as a unity for the understanding, the formal intuition of space is an intuition of it *considered as though it were an object.* The kind of entity that can be considered an object while providing the necessary background for all objects of a given sense (here, outer sense), which is categorially determined exactly insofar as this enables the determination of particular objects within it, is what we could call a *horizonal object.* With such a notion, Kant's theory of space can properly be described as proto-phenomenological.[68]

Turning now to the second issue (problem 3), namely, Kant's assertion that the unity of the formal intuition of space is thereby also described as *belonging* to space, rather than to "the concept" (B161n), this issue is similarly a consequence of the nature of the categorial determination of space. As we have seen, the formal intuition of space is produced through the synthesis of apprehension of the unicity of space. The synthesis of apprehension unites the sensible manifold *in sensibility*, "in an empirical intuition" (B160), so that space as formal intuition is determined "*in accordance with the categories*" (B152) and at the same time is an intuition that must be regarded as *given* and as determined in its *givenness* rather than generated by the understanding ex nihilo, as it were. That is to say, the determination of space by virtue of the synthesis of apprehension (that is, the productive imagination in the domain of the sensible manifold) concerns the determination of the given manifold as objective, and so the generation of determinate spaces. It thus becomes clear that there is no discrepancy between the merely given manifold (space as form of intuition) and the determined manifold as constituting the formal intuition: both concern the same given manifold of representations, with the difference being that space as formal intuition concerns a unified intuition in contrast to the mere manifold of space as form of intuition. It is in this sense that the unity of space as formal intuition, although effected by the understanding by virtue of the imagination, *belongs* to the intuition, rather than to the categories or the concepts of the understanding, as Kant says in the footnote.

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[67] Here, we note Fichant' s (2004, 548) denial that there is such a concept. We agree that this notion of "concept" of space is not very useful, but still think it can be used even if it has only one referent. A similar uneasiness with the notion of concept can be found in Shabel (2003, 49), but the notion of "aesthetic concept" she introduces as an alternative is left quite vague.

[68] In so doing, the suggestion is that Kant would have benefitted from having access to something like the phenomenological notion of "horizon" (see Welton 2000, 78) to clarify how space can be viewed as an object.

At the end of the footnote, Kant refers back to section 24, where indeed it is said that "the imagination, on account of the subjective condition under which alone it can give a corresponding intuition to the concepts of understanding, belongs to *sensibility*" (B151). Since the unity is imposed on the manifold by means of the imagination, and the imagination belongs to sensibility (or the empirical intuition), the unity of the formal intuition also belongs to the intuition and not to the (mere) concepts of the understanding. This is confirmed by Kant's assertion that "as figurative, [the synthesis of the imagination] is distinct from the intellectual synthesis without any imagination merely through the understanding" (B152). The distinction between the formal intuition, which, "as the understanding determines sensibility," "gives unity of the representation" (B160–61n), and the form of intuition, which "merely gives the manifold" (B160n), corresponds to the distinction, in section 24, between the imagination as "an exercise of spontaneity, which is *determining*," and sensibility as such, which is "merely *determinable*" (B151–52; emphasis added). The quintessentially transcendental distinction between the receptivity of a sensible manifold and the spontaneity of the act of the synthesis is thus reflected in the very distinction, in the footnote, between space (and time) as the form(s) of intuition and as formal intuition(s). Correlatively, one can see why the unity of space cannot belong "to the concept of the understanding" (B161n): the determination of space under the categories does not identify it as an instance of a general concept since space is essentially unique.[69]

However, the unity of space under TUA is not that of space as a form of intuition on our interpretation: it is only insofar as the unicity of space, which characterizes space as a form of intuition, is taken as a unity that we get to obtain the unity of space as formal intuition referred to in B161n. This explains why Kant also says at B161 that the unity of space is given *with* the intuition, not in it. That is, the unity of which Kant is speaking here is the unity that is effected in the sensible manifold, in accordance with TUA, and is conditional on an act of synthesis. This unity, as result of an act of synthesis, is not the unity of space, as form of intuition, which we have called the *unicity* of space and can be described as *internal* to space. In this way, the text of the footnote supports our nonconceptual interpretation that emphasizes the fact that the mere manifold of space, as a form of intuition, is independent of TUA.

There is more to say about the unity of space as formal intuition in regard to its role in Kant's theory of the object. In the A-version of TD, Kant explains how representations have their object and how appearances, which are "objects that can be given to us immediately" (A108) through intuition,[70] are themselves only representations that refer to an object that we cannot intuit, the "transcendental object = X" (A109). It is through the pure concept of the transcendental object that our empirical concepts relate to an object. But what ensures that concepts relate to particulars? It is the fact that we are related immediately to the object as located in a domain of the particular. For outer sense, this domain is space, and it is by grasping space in its unicity that we grasp the object as belonging to this single domain of all particulars of outer sense. Reference to a *particular* is not achieved through the particular set of conceptual determinations. These only enable the object to be identified in terms of the set of properties corresponding to these determinations. What first enables reference to a particular is the location of the object in space (and time). Space, as formal intuition, is a representation of the domain of all particulars in outer sense *as* particulars. Space is necessarily underdetermined because particulars *qua* particulars cannot be grasped conceptually (the thoroughgoing determination of an object defines merely a regulative ideal; see A571–72/B599–600); and space is an intuition, because grasping any particular as particular means grasping it by means of an intuition, which must be spatial if it is a particular of outer sense.

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[69] Compare again Refl. 4756 (from around 1775–77), where Kant gives as one of the characteristics of space: "Unity, hence a pure intuition and not a concept of the understanding" (AA 17:700 [Kant 2005, 181]).

[70] We recall that for Kant an intuition enables us to have an immediate representation of a particular, whereas concepts, as general representations, are always only mediately (via intuition or another concept) related to a particular (A68/B93; A320/B377).

It might be objected that it is possible to grasp a particular Roger Bannister under a singular concept; but we would argue, with Kant, that to grasp a particular under a singular concept does not constitute a grasp of that particular *as* a particular. Indeed, consider the grasp of Roger Bannister as "the first man to run a mile under four minutes." This does actually identify a singular object, but prior conditions exist for the concept attributable to this singular object to be singular. These are that there is a unique time, that is, that there is a unique referent of "first" (there are no other temporal dimensions in which there would also be other men with the same property) and a unique space (note that distinct spaces would have unrelated times, else a notion of superspace including them all would be implied, namely, the superspace of all simultaneous events in outer spaces, or at least one could argue this along the lines of Kant's arguments in TAe). Consequently, if we are to grasp Roger Bannister as a particular, we must have an intuition in space and time,[71] so that the unicity of space and time are part of this grasp. Because Kant characterizes intuitions as immediate representations, this means that this grasp must be immediate.[72]

Another important feature of the formal intuition of space is that it provides us with some basis for the axioms of geometry (and we note that there is nothing specifically Euclidean about this basis).[73] That is, insofar as geometry deals with objects (*quanta*), as opposed to arithmetic and algebra that deal with magnitude in general (*quantitas*), it requires axioms (Friedman 1992, 114). The topological features of space provide the tools for carrying out geometric constructions (see Fichant 2004, 548–49; Shabel 2003), while the properties of homogeneity (in translation and rotation) enable the formulation of postulates (for example, Euclid's first four postulates), which ensure the existence of points defined by these constructions (Sandmel 2008, 673).[74]

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[71] This intuition may be *of* Roger Bannister, or of any other state of affairs that we thereby take to be connected to the existence of Roger Bannister through some causal chain. This chain could involve reliable witnesses or empirical laws.

[72] We thank an anonymous reviewer whose example we borrowed for getting us to think more carefully about this issue.

[73] With reference to the passage discussed earlier, namely, B39/A25, where Kant asserts that space is not a discursive representation but a pure intuition, for "one can only represent a single [*einigen*] space, and if one speaks of many spaces, one understands by that only parts of one and the same unique space" and that "the manifold in [single space] . . . rests merely on limitations," Marcucci (2010, 38) notes that since the single space, as intuition, lies at the basis of determinate, circumscribed spatial regions, "it itself *cannot to the extent that it is an intuition have any dimensions*"; space as intuition rather "engenders [*dare origine*]" an "inestimable number of circumscribed spaces, which are precisely spaces with 1, 2, 3, . . . , n dimensions, exactly as is required when one talks of

Conclusion

We have proposed an interpretation centered around a nonconceptualist approach to interpreting B160–61n, with the following specific claims:

1. Kant provides a properly phenomenological analysis of the form of our receptivity or outer sense that defines a clear notion of nonconceptual unity, namely, the unicity (defined by *singularity*, *infinity*, and *mereological inversion*) of the form of spatial intuition. This form has topological properties (*continuity*, *externality*, *centered externality*, and *homogeneity*), which account for the role of the unicity of space as the form of outer sense and as ground for geometric constructions.
2. To consider space as an object, as Kant does in the footnote, is to consider what and how this form of intuition contributes

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curved space, no longer bound and conditioned by Euclidean geometry" (our translation). This suggests that Kantian space as pure intuition cannot be Euclidean, and is thus not vulnerable to being superseded by the discovery of non-Euclidean conceptions of space. From early on, Kant left open the possibility of multidimensional spaces, which would be studied by what, in *Living Forces*, Kant's first published work (1747), he calls "the highest geometry" (LF, AA 1:24–25). Unlike many present-day uses of the word "space," Kant's notion of space in TAe refers to a nonconceptual notion of spatiality that is amenable to different pairwise inconsistent conceptualizations, Euclidean or not, as Risjord (1990, 136) argues.

[74] *Pace* Sandmel (2008), we do not include the property of homogeneity with respect to scale (equivalent to Euclid's fifth postulate), as this property is not needed to ensure the possibility of extending a line segment *by a given amount* (homogeneity in translation) or drawing a circle *with a given radius* (homogeneity in rotation) around a point (Friedman 1992, 86). A Euclidean (or quasi-Euclidean) geometry is arguably given a privileged status on grounds of simplicity. Its applicability to perception does not invalidate this a priori choice. We follow Sandmel, however, in viewing "modern 'geometries' [as] . . . not [being] geometries at all in the sense of their being theories of the space of our outer intuitions" (Sandmel 2008, 678).

to objective knowledge, namely, by regarding the unicity of space *as* a unity. This involves the pure synthesis of apprehension (the transcendental synthesis of the imagination in the domain of perceptions) that defines space as formal intuition. Space is thus determined through the categories, but without thereby losing its status as an intuition.

3. The determination of space has the function of enabling the representation of objects of outer sense *qua* particulars. The imagination plays the key role here, without its synthetic activity being independent of the understanding (unlike Heidegger's interpretation of the imagination as a quasi-autonomous faculty characterizing our human finitude). With Fichant and against Longuenesse and Friedman, we however maintain that a key characteristic of space is its nonconceptual unity, that is, the unicity of space, which is independent of the synthesis of the imagination and thus of determination by the understanding.

While we therefore uphold the existence of a nonconceptual unity of space, that is, the unicity of space, which is key to understanding how we can have knowledge of spatial appearances, this nonconceptual unity of space is a necessary but not a sufficient condition for such knowledge. That is to say, it is only insofar as the unicity of space is *taken as a unity* through a synthesis of the imagination in accordance with the categories that we can have spatial knowledge. This means that, while our proposed interpretation shows the independent function of a nonconceptual unicity of space as a condition of empirical knowledge, such nonconceptual content is relevant to the acquisition of objective knowledge only when determined in accordance with the functions of the understanding by means of the synthesis of the imagination. However, unlike conceptualist interpretations of the notion of "form of intuition," in our reading the unicity of space does not collapse into the synthetic unity of space, and hence into sensible content determined in accordance with the categories, which corresponds to space as formal intuition.

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Names Are Predicates

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Names are predicates. This claim can mean different things to different people. Willard V. Quine thought that names were predicates in the sense that they were to be represented as general terms in translations of English sentences into the language of first-order logic.[1] Clarence Sloat (1969) thought that "proper nouns," as he called them, were predicates in the sense that, like common count nouns, they must occur either as a bare plural or with a determiner (such as 'the', 'some', and 'a'), although 'the' would in some cases go unpronounced. Tyler Burge thought that "proper names," as he called them, were predicates in the sense that they

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[1] In some works, Quine (1940, 1948, 1950) proposed that names should be translated as definite descriptions with the name itself showing up in the predicate component of that description. In *Word and Object*, Quine (1960) thought, in contrast with his earlier view, that names were predicates in their own right, not just in his regimented language.

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were true or false *of* things and should be treated as general terms in a semantic theory of English.[2]

While I agree with all of these claims, when I say that names are predicates, I mean that they have a predicate-type semantic value, whatever that might turn out to be. The semantic value of a predicate might be extensional: for example, a function from entities to truth values (a type-$\langle e, t\rangle$ function). Or it might be intensional: for example, a property, or a function from possible worlds to extensions.

The question which of these types might be a semantic value of a predicate is independent of the question whether names are predicates. If names are predicates, then whichever of the above semantic types that can be a value of the predicate 'planet' can also be a value of the name 'Aristotle' and vice versa.

In what follows, I assemble a variety of arguments in order to make the beginnings of the best case that I can for *Predicativism*: the view that names are predicates in all of their occurrences.[3] When names appear as bare singulars in argument position, they constitute the predicative component of a *denuded determiner phrase*, a determiner phrase with an unpronounced determiner.[4]

Burge proposed that the unpronounced determiner was a demonstrative, so that bare singular names, apparently in argument positions, would constitute the predicate component of a denuded complex demonstrative.[5]

I endorse an alternative version of Predicativism, one that has already been advocated by a number of authors: the unpronounced determiner is the definite article, so that bare singular names actually constitute the predicative component of a denuded definite description.[6]

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[2] See Burge 1973.

[3] Inevitably, the fullest case cannot be made within the confines of a single article. For further arguments, see Fara forthcoming, n.d.

[4] I do not mean here to take a stand on the "DP hypothesis" (Abney 1987). I speak of "determiner phrases" rather than "noun phrases" for the sake of familiarity and ease of presentation.

[5] It's not clear that Burge himself would accept this formulation of his view, but I will ignore that interpretive question here. Advocates of Burge's version of Predicativism include Reinaldo Elugardo (2002) and Sarah Sawyer (2010).

[6] In addition to Sloat (1969), see also Richard Larson and Gabriel Segal (1995), Bart Geurts (1997), Paul Elbourne (2005), Ora Matushansky (2006b, 2008), Yu Izumi (2012), and Aidan Gray (2013).

The argument begins by reviewing familiar evidence that names are count-noun predicates in at least some of their occurrences. In addition to its thesis about the semantic *type* of names, Predicativism includes a thesis about their semantic applicability condition: names are true of their bearers. In sections 2 and 3, I explain this condition—*the being-called condition*—and demonstrate that it can be given a nonmetalinguistic formulation. In section 4, I set out the denuded definite descriptions view of bare singular names. Then in sections 5 and 6, I explain Burge's 'That'-Predicativism and defend it against certain objections that would also serve against 'The'-Predicativism. Most of the positive case is located in sections 9, 10, and 11, where I appeal to some known syntactic facts in order to demonstrate that 'The'-Predicativism is not only preferable to 'That'-Predicativism but is also simpler and more explanatorily powerful than its main competitors; and in section 13, where I explain why 'The'-Predicativism survives Kripke's modal argument. I conclude with a summary and some methodological remarks.

1. Names Are Count Nouns

Some predicativists about names are moved by examples of the following sort.[7]

(1) There is one Alfred in Princeton (based on Burge 1973).
(2) Every Sarah I've met sometimes works as a babysitter.
(3) Sarahs from Alaska are usually scary.

In these sentences, names occur as a complement to a quantificational or a numeric determiner or as a bare plural. In each case, we have a name occurring in a position that is typically occupied by common count nouns like 'hunter' and 'bear'. Just as we can count how many hunters or bears there are in Princeton or Alaska, we can count how many Alfreds or Sarahs there are in Princeton or Alaska. Just as we can make generalizations about hunters or bears from Alaska, we can make generalizations about Sarahs from Alaska.

Frege thought that names have descriptive, predicative conditions "associated" with them.[8] Despite this, Frege—like Mill before him and

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[7] In addition to Burge (1973), see Jennifer Hornsby (1976), Elugardo (2002), Elbourne (2005), Nick Kroll (n.d.), Paul Pietroski (2007), and Sawyer (2010). Larson and Segal (1995) don't explicitly adopt the predicate view, but they find it an attractive alternative to orthodoxy. Segal (2001) later reconsiders but (sort of) rejects it.

[8] See Frege (1948 [1892], 210).

Kripke and Kaplan after him—thought that names were not predicates but referring expressions, by which I mean expressions with an individual as semantic value, that is, expressions of type *e*.[9] Since Frege, Kripke, and Kaplan never explicitly considered cases where names occurred in the plural, or with determiners, they didn't qualify their claims that names had individuals as their semantic values. Thus we could call the view that names always have an individual as a semantic value the Frege-Kripke view of names. The point in so doing would be to emphasize the diversity of the views that I'm rejecting.[10] The debate between Fregean descriptivists on the one hand and Millian or direct-reference theorists on the other is a debate that is properly understood as a debate between two different referentialist camps.

There is another way to carve up the referentialist view, one that's orthogonal to the divide between descriptivists and Millian or direct-reference theorists. I take *Referentialism* to be the view that when names occur as bare singulars in argument position, they are referring expressions, not predicates. There is no way to accommodate names in predicate position as normal referring (type *e*) expressions. So referentialists may be either (i) *uniform referentialists*, who say that occurrences of names in predicate positions are nonliteral or perhaps deviantly derived from the more basic singular-term use, as verbs are sometimes derived from nouns or adjectives, a shift objected to by linguistic conservatives—as when 'express' is now used as a verb ("This train is expressing to New York") derived from an adjective ("This is the express train to New York") or (ii) *nonuniform referentialists*, who say that when names occur as bare singulars in argument position, they are referring expressions, but when they occur otherwise, they are literal, perhaps nonderived predicate expressions.

My view is that names are predicates in all of their occurrences, even when they appear to be bare singulars in argument position. I call this view *Predicativism*. In all of their occurrences, names have a predicate-type semantic value. My arguments for Predicativism do not depend on details that distinguish the two versions of Referentialism. Thus the difference between Uniform and Nonuniform Referentialism (if there is such a difference) is not one that matters to me here. Thus I will use the term 'Referentialism' to include both the uniform and nonuniform versions.

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[9] See Frege (1948 [1892]), Kripke (1980 [1972]), and Kaplan (1989).

[10] But in so doing, I do not mean to suggest that the view, as I just stated it, can be found in the writings of Frege, Kripke, or Kaplan.

Here are two more examples from Burge in which it is uncontroversial that names are occurring as predicates.[11]

(4) An Alfred who joined the club today was a baboon.
(5) Some Alfreds are crazy; some are sane.

The names in these sentences occur as count nouns: in the first case, in the singular as a complement to the indefinite article, and in the second case, in the plural.

The following examples suggest that names do not naturally occur as mass nouns.

(6) a. There is not much dust in Princeton.
b. * There is not much Alfred in Princeton.
(7) a. Most dust is allergenic.
b. * Most Alfred is allergenic.

2. Names Are True of Their Bearers

There's no controverting Burge's view that names occur at least sometimes as predicates with multiple application. In these occurrences, they are predicates that are true of their bearers. The predicate 'Tyler' is true of both Tyler Burge and Tyler Doggett; this is partly why (8) is true.

(8) There are at least two Tylers with philosophy degrees from Princeton.

'Tyler', in 'at least two Tylers', can be occurring only as a predicate, given that it occurs in the plural as the complement of the complex numerical determiner 'at least two'. Further, this predicate 'Tyler' has multiple application, since there can be at least two Tylers from Princeton only if the predicate 'Tyler' applies to at least two things. Finally, and no less obviously, this predicate 'Tyler' applies to all the things called Tyler, and to them only; if there weren't two people *called* Tyler with philosophy degrees from Princeton, the sentence would not be true.

Let me strengthen these last two claims by saying that names in these positions satisfy the *being-called condition.* It's best to represent this condition as a schema, where the schematic letter, capital en, is to be replaced by a name.

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[11] Uniform referentialists could coherently say that these predicate occurrences are derived from standard occurrences, on a par with 'fax', 'contact', or 'express' when they occur as verbized nouns.

(*BCC*) '*N*' (when a predicate) is true of a thing just in case it is called *N*.

I claim that in every case where a word occurs as a name, it is a predicate with (potentially) multiple application. The condition of its application is given by the *being-called condition.* (Of course I don't mean that the condition holds in cases where names are used metaphorically, with deferred interpretation [à la Nunberg 1995, 2004], or metonymically in any way, any more than I would insist on the standard application conditions for other predicates when used in one of those ways.)

The being-called condition was just expressed as a schema. We write down an instance of that schema by writing it down with both occurrences of its schematic letter, capital en, replaced by a word of the indicated category. In this case, it is names that are in the indicated category. Here, then, are some instances of the schema:

(9) 'Aristotle' (when a predicate) is true of a thing just in case it is called Aristotle.
(10) 'Tyler' (when a predicate) is true of a thing just in case it is called Tyler.

The being-called condition is to be distinguished from a different but related condition, which I call the *bastardized being-called condition,* expressed as a universal quantification.

(*BBCC*) For every name *N*, *N* (when a predicate) is true of a thing just in case it is called *N*.

Here, the capital letter en is not a schematic letter but rather an objectual variable bound by a first-order universal quantifier. To make this more perspicuous, we might use a lowercase letter instead of an uppercase one as our variable:

(*BBCC*) For every name *x*, *x* (when a predicate) is true of a thing just in case it is called *x*.

Here are some instances of the bastardized being-called condition:

(11) 'Aristotle' (when a predicate) is true of a thing just in case it is called 'Aristotle'.
(12) 'Tyler' (when a predicate) is true of a thing just in case it is called 'Tyler'.

One difference between the two conditions is obvious: in the being-called condition the name 'Alfred' is used, while in the bastardized being-called condition the name 'Alfred' is mentioned. An instance of the bastardized being-called condition can contain only a quoted name after the past participle 'called', while an instance of the being-called condition proper may contain an unquoted name after 'called'.[12] This is precisely why our version of the being-called condition must be expressed as a schema.

A related difference between the two is that the passive verb form 'is called' in the bastardized condition is followed by an individual-level objectual variable and must therefore be a relation expression with at least two arguments: a grammatical subject (that which is called something) and an object (what the subject is called). Meanwhile, the past participle 'called' in the being-called condition proper is not followed by an individual-level objectual variable, but rather by a schematic letter that is to be replaced by a name, which may in turn be unquoted. On my view, these are occurrences of names as predicates. So on my view, the passive verb 'called' is not being treated as a relation expression but rather a bit more like a copula—like 'turned' in 'he was turned green by the witch'. If this view of 'called' is correct, then the bastardized being-called condition is ungrammatical since it would have an individual-level variable occurring in a predicative position. It would be like writing '*x* was turned *y* (by the witch)'.

On the flip side, if a 'called' predication could be true only if 'called' were followed by a quote-name, as in "Socrates was called 'Socrates'," then no instance of the being-called condition proper would be true. From the perspective of a proponent of the bastardized being-called condition, instances of the being-called condition proper are roughly incoherent. On their view, to say that Obama is called Barack is not to say that Obama has a certain name, but rather that Obama is called a certain person—himself.

3. Names Are Predicates in the Being-Called Condition

We had said (schematically) that a name '*N*', when it occurs as a predicate, is true of a thing just in case that thing is called *N*. We called this the being-called condition. Using double-bracket notation to represent the function from a predicate to its extension, we state the being-called condition (schematically) as follows:

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[12] For ease of exposition, I'm pretending that putting a name in quotes is the only way to form a name for it.

(13) When 'N' occurs as a predicate, $[\![\text{'}N\text{'}]\!] = \{x : x \text{ is called } N\}$.

The grammaticality of (13) turns on whether '*N*', in '*x* is called *N*', is in a predicate position. This is the position of the predicate 'supportive' in '*x* called *y* supportive' or that of 'a saint' in 'Erica called Lorenzo a saint'. In 'Erica called Lorenzo supportive', 'Lorenzo supportive' is a *small clause*, a subject-predicate construction with no finite verb to connect the subject with the predicate.[13] In 'Erica called Lorenzo a saint', 'Lorenzo a saint' is a small clause.

(14) Erica called $\underbrace{\text{Lorenzo supportive}}_{\text{small clause}}$.

(15) Erica called $\underbrace{\text{Lorenzo a saint}}_{\text{small clause}}$.

'Called' is not the only verb to take a small-clause complement. In 'Erica made Lorenzo mad', 'Lorenzo mad' is a small clause.

(16) Erica made $\underbrace{\text{Lorenzo mad}}_{\text{small clause}}$.

In 'Lorenzo drunk himself silly', 'himself silly' is a small clause with a reflexive pronoun in its subject position.

(17) Lorenzo drunk $\underbrace{\text{himself silly}}_{\text{small clause}}$.

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[13] For syntacticians, to say that these strings constitute small clauses is to say that they form a syntactic constituent. Whether this is so has been a matter of debate. Effectively, the question is whether 'Lorenzo' in (14) is a syntactic subject of the "small clause," and if so, in what sense formally? But that debate is not relevant for us here. The position occupied by 'supportive' in (14) is uncontroversially a predicate position, and the position occupied by 'Lorenzo' is uncontroversially in a position to be occupied by the *semantic* subject of that predication. That is all that matters for us. So when I say that a phrase is a small clause, I can be interpreted as saying that it would be called a small clause by the syntacticians who advocate a small-clause analysis of these constructions.

For significant early discussions in developing and promoting small clauses, see Tim Stowell (1981, 1983). For a briefer early defense of small clauses, see Ken Safir (1983). See Edwin Williams (1983) for some objections to small clauses. And Stowell (1995) provides a useful overview.

The claim here is that in 'My parents called me Delia', 'me Delia' is a small clause.[14]

The pronoun 'me' occurs in its subject position, and the name 'Delia' occurs in its predicate position.

(18) My parents called $\underbrace{\text{me Delia}}_{\text{small clause}}$.

Ora Matushansky (2005, 2008) has supported the latter claim by pointing out, among many other facts, the following: if appellative 'call' were a verb that allowed for two objects—as it would have to if the bastardized being-called condition were grammatical—then it should be passivizable in both of its object positions. But it is not. Contrast the active-passive counterparts in (19) with those in (20) and (21).[15]

A paradigm double-object verb, such as 'give', is passivizable in both of its object arguments.

(19) a. Maria gave Carlos a book.
b. Carlos was given a book by Maria.
c. A book was given (to) Carlos by Maria.

But appellative 'called' does not have this property.

(20) a. My parents called me Delia.
b. I was called Delia by my parents.
c. *Delia was called of/to/for me by my parents.

Not surprisingly, we see the same pattern with appellative 'called' with an adjective in its appellation position.

(21) a. Erica called Lorenzo supportive.
b. Lorenzo was called supportive by Erica.
c. *Supportive was called Lorenzo by Erica.

So on the right-hand side of the being-called condition, the name '*N*' occurs in the predicate position of a small clause. For example, 'Willard' is in a predicate position in the sentence 'Hattie called Quine Willard'.[16]

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[14] See Matushansky (2005) for defense of this latter claim.

[15] In the remainder of this section, I merely elaborate this one particular argument of Matushansky's. She has a number of others.

[16] I should say that while the failure of the passivizability of 'Willard' here is a necessary condition for the small-clause analysis, it is not a sufficient condition. There may be failures of passivizability in the case of arguments, for example, measure phrases in a sentence such as 'Jon Rauch is seven feet tall'. (Thanks to Ora Matushansky for pointing

I use the term "appellative 'call'" for occurrences like those in (14), (15), and (18). By *appellative 'call'* I mean the verb that we use when we say that someone has called someone something. My father called me Delia; my father called me stubborn. We naturally say, in each case, that my father called me something. Both cases involve what I'm calling appellative 'call'.[17] Here's one proposal of a semantic analysis for appellative 'call': 'x called y F' is true just in case x said or did something that entails or presupposes that y is F.[18] This proposal warrants further discussion, but I will not pursue that here.

I contrast appellative 'call' with "summoning 'call'," which is the verb that we use when we say that someone called someone or that someone called something or someone *for* someone. (Summoning includes telephoning.) I use "appellative 'call'" to exclude *summoning 'call'* but to include the *calling* that we normally describe as *calling someone names.* In the everyday sense, if I've called you scrawny and lazy, then I've called you names. So that is a use of appellative 'call'. Unlike appellative 'call', summoning 'call' is passivizable in any of its object positions.

(22) Single-object summoning 'call':
  a. Maria called Pilar at three o'clock.
  b. Pilar was called by Maria at three o'clock.
(23) Double-object summoning 'call':
  a. Choate called Howlett a cab.
  b. Howlett was called a cab by Choate.
  c. A cab was called for Howlett by Choate.

Have you heard the old joke about calling someone a cab?

HOWLETT: Choate, will you call me a cab?

CHOATE: OK, you're a cab.

The joke plays on the homonymy of the two 'call's. Howlett's request is that a cab be summoned for him by Choate. If Choate had called Howlett

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this out to me.) But there is not consensus about whether measure phrases are arguments. See Schwarzschild (2005) for arguments against.

[17] Think of it this way. I use 'something' here the way we use 'everything' when we say "Jack has been everything" to imply that he's been a carpenter, he's been an actor, he's been a waiter, he's been a teacher, and so on.

[18] See, by way of comparison, Fara (2011), where it is proposed that 'x called y F' is true just in case x said or did something that entails that y is F (no allowance for presupposition).

a cab in that sense, then we could say, in the passive voice, both (23b), that Howlett was called a cab by Choate and (23c), that a cab was called for Howlett by Choate. But given what Choate in fact did, namely, say to Howlett that he *is* a cab, the passive voice allows us only to say the former, (23b): that Howlett was called a cab by Choate.

In other words, if Choate had done what Howlett wanted him to do, then all of (23a–c) would have come to be true. But given what Choate in fact did, (23c) did not come to be true; only (23a–b) did. When we say that Choate called Howlett a cab, the indefinite description 'a cab' is in the direct-object position of a double-object verb if we're using summoning 'called'. But if we're using appellative 'called', then the description 'a cab' is in the predicate position of a small clause. Thus passivizability from the direct-object position of summoning 'called' distinguishes the two.

4. Names Appearing to Be in Argument Position Are Embedded in Denuded Definite Descriptions

Names do not always appear in predicate positions. They also sometimes appear as bare singulars in argument positions: as subjects, direct objects, indirect objects, and objects of prepositions.

(24) Venus is celestial.
(25) Hattie loved Willard.
(26) Hattie gave Willard a book.
(27) Marjorie went to the restaurant with Willard.

The names here do not appear to be in predicate positions. Abstraction from these positions yields perfectly good propositional functions, in Russell's (1903) sense.

(24′) *x* is celestial.
(25′) *x* loved *y*.
(26′) *x* gave *y* a book.
(27′) *x* went to the restaurant with *y*.

It is natural to adopt Referentialism in light of commonplace examples like these. Frege's view in "Sense and Reference" was that the "Bedeutung" (that is, the *semantic value*) of 'Venus' is an individual, and the Bedeutung of the unsaturated predicate '___ is celestial' is a function, which when supplied with an individual as argument yields THE TRUE or THE FALSE as a value. In other words, the semantic value of 'Venus' is an entity on Frege's view, and the semantic value of '___ is celestial' is a function of type $\langle e, t\rangle$. The sentence (24) is true since the semantic value of '___ is celestial' yields THE TRUE when applied to the entity Venus, the semantic value of 'Venus'.

This is a pleasingly simple semantic analysis of subject-predicate sentences with names as subjects. More than one well-known philosopher of language has said to me that names in predicate position are just clearly used differently from names in argument position; in predicate position a name *N* is a "metalinguistic" predicate, true of just those things that have *N* as a name. The suggestion is that we can keep the Frege-Kaplan view of names and just set aside predicative names as requiring no more discussion than that.

I take a different approach. I do not think we should set aside names in predicate position as ubiquitous deviants. We would do best to try to develop a unified theory of names appearing in predicate positions and in argument positions. Moreover, I take this approach from the opposite direction. I begin with an account of names appearing in predicate position. Subsequently, I develop that account so that it extends to names appearing to be in argument positions. I take the analysis of names as multiply applicable predicates as a starting point and proceed from there. The question facing us now, then, is this: if names are predicates, not referring expressions, not, that is, expressions with an individual as a semantic value, not, that is, expressions of type *e*, then how could a name compose with an individual-level predicate to yield a truth value?

According to the theory I'll maintain, when a bare singular name appears in argument position, it is the predicative component of a *denuded definite description*, by which I mean a definite description with an unpronounced definite article.[19] The view is related to, but not the same as, Kent Bach's (1987, 2002) "Nominal Description Theory." According to Bach, a name '*N*' is "semantically equivalent" to the description 'the bearer of '*N*''.[20] This is different from the version of Predicativism defended here, though. According to the being-called condition, a name '*N*' is semantically equivalent to the predicate 'thing called *N*'. This latter predicate is equivalent to 'bearer of '*N*'' (see note 26), so for us, it is '$\emptyset_{the}$ *N*' that is equivalent to 'the bearer of '*N*''.

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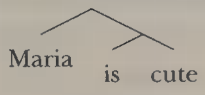
[19] This view is held by Elbourne (2005), Izumi (2012), Larson and Segal (1995), Matushansky (2006b, 2008), and Sloat (1969).

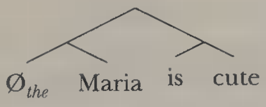
[20] Others who could be classed with Bach here are William Kneale (1962), Geurts (1997), and Jerrold Katz (2001).

What appears on the surface as (28) is underlyingly really (29).[21] ('$\emptyset_{the}$' is the unpronounced definite article.)

(28)



(29)



On the view I endorse, therefore, the compositional semantics and the syntactic form of sentences containing names will be special cases of sentences containing other descriptions, numerical phrases, quantified noun phrases, complex demonstratives, or bare plurals. Whatever syntactic form the (a) sentences below have, the (b) sentences have that form as well. In whatever way the elements of the (a) sentences formally compose to yield a proposition or truth value, the corresponding elements of the (b) sentences formally compose in that way too.

(30) a. The table is tall.
b. $\emptyset_{the}$ Maria is tall.
(31) a. The table in my room is tall.
b. The Maria in my room is tall.
(32) a. The lady was buying one desk while seated at another.
b. $\emptyset_{the}$ Jacquie was reading one Aristotle while married to another.
(33) a. Some people read history, others make it.
b. Some Williams read history, others make it.
(34) a. Bears from the north are usually scary.
b. Sarahs from $\emptyset_{the}$ Alaska are usually scary.
(35) a. That man hates to swim.
b. That Maria hates to swim.
(36) a. That man sure does love to swim.
b. That Natalia sure does love to swim.

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[21] In a number of natural languages, definite articles before unmodified singular names can, at least sometimes, get pronounced. Examples are Portuguese, Modern Greek, French, German, Italian, and Spanish. See Matushansky (2006b, 2008) for extensive cross-linguistic and intralinguistic discussion of the different strictures of different languages on the occurrence of an overt definite article with unmodified singular names. This is a standard sort of evidence for the proposed phonologically null definite article in English when it is in that same position.

(37) a. The book was laid on the table.
b. The book was written by $\varnothing_{the}$ Kripke.
(38) a. Every woman who owns a donkey beats it.
b. Every woman who knows an Agatha loves her.
(39) a. If Lexi buys a horse and rides it, then her donkeys will be upset and kick the horse as often as they can.
b. If Alexa meets a Bucephalus and marries him, then her parents will be upset and insult $\varnothing_{the}$ Bucephalus as often as they can.[22]

In each case, the semantic types of the expressions in the (b) sentences will be the same as those of the corresponding expressions in the (a) sentences. Since the common nouns in the (a) sentences all have predicate-type semantic values, the names in the (b) sentences all do as well. The truth values of, and the propositions expressed by, the (b) sentences will therefore be determined in the same compositional way as those of the (a) sentences.

Because my view of the syntax and compositional semantics of names is parasitic on the syntax and semantics of other count nouns, I do not need to provide detailed investigation or elaboration of their syntax and compositional semantics in order to present my view. In general I entitle myself to say here just that the syntax and compositional semantics of names piggybacks on those of other count nouns. But let me clarify two points.

First, in saying that the syntax of names piggybacks on that of common nouns, I do not mean to say that there is no syntactic difference between names and common nouns. There must be *some* syntactic difference between them. Otherwise we would not have 'the' being permitted to occur before the noun in 'the man is on the road' but prohibited (in English) from occurring before the name in 'Pierre is on the road'. I mean rather to be making conditional claims like the following: if the bare-plural construction in 'bears are scary' contains a syntactically real but unpronounced generic operator, then so does the bare-plural construction in 'Sarahs are scary'; if the semantic value of 'bears' in 'bears are scary' is the set of all sums of individual bears, then the semantic value of 'Sarahs' in 'Sarahs are scary' is the set of all sums of individual Sarahs.[23]

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[22] This sentence is based on ones from Elbourne (2005, 182), 'If John insists on calling his next son Gerontius, then his wife will be annoyed and Gerontius will get made fun of because of his name', and Geurts (1997, 322), "If a child is christened 'Bambi', and Disney Inc. hear about it, then they will sue Bambi's parents".

Second, in saying that the compositional semantics of names piggybacks on that of common nouns, I mean only to say that names and common nouns have the same type of semantic value, not that they have the same conditions of application. The truth conditions of sentences containing the one are compositionally determined in just the same way as those containing the other. For example, as I said at the outset, I would take names to have the same predicate-type semantic value that other common nouns have, whatever that might be. Wherever common nouns are best treated as having an extensional semantic value, names are too. Wherever common nouns are best treated as having an intensional semantic value, names are too. Piggybacking does not, in contrast, require that names and other common nouns have the very same form of application conditions.

The name 'Michael' applies to a thing just in case it is called Michael. It does not follow that the common noun 'sage' applies to a thing just in case it is called a sage. My parents called me a sage; that did not make me one. In contrast, my parents did call me Delia, and that did make me a Delia. The relevant sense of 'called' here is not the one used in disquotational truth and application conditions, *pace* Kripke (1980 [1972], 68–70). We are not saying that to be called Michael *just is*, with nothing more to be said, to be in the extension of the name 'Michael'. Rather, being called 'Michael'—being in the extension of the name 'Michael'—is something like having been dubbed Michael, with the name 'Michael' continuing to be used to refer to you in ways that are caused, with a chain of intentionally reference-preserving links, by the original dubbing. Thus Kripke's "causal theory of reference" could well be maintained as an analysis of *what it is* to be called Michael—to be in the extension of the name 'Michael'.[24]

5. Burge Said 'That'

Burge (1973, 430) advocated a version of the being-called condition: "A proper name is (literally) true of an object just in case that object is given the name in an appropriate way." This invites the question: *appropriate for*

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[23] This view of plurals as true of "sums" was proposed by Richard Sharvy (1980).

[24] For a recent investigation of the nature of the property of bearing a particular name, see Gray (2013).

what? I presume that Burge intended to rule out cases in which you're given a name without that name becoming a name *of yours*. For example, in order to preserve anonymity, the doctors gave their patient the pseudonym 'Linda'. But the patient was not given the name 'Linda' in the appropriate way, in Burge's sense, because the name did not thereby become a name *of hers*.[25] So although Burge's criterion does not overtly involve the notion of being called something, I consider it to be a version of the being-called condition since in my view for a thing to be called *N* just is for it to have been given the name '*N*' in such a way that '*N*' becomes a name *of its*. This in turn is equivalent to being a bearer of the name '*N*'. So to be called *N* is to have been given the name '*N*' in an "appropriate way"—that is, to have '*N*' be a name of yours—which in turn is to be a bearer of the name '*N*'.[26]

Since Burge accepted (his own version of) the being-called condition for all occurrences of names—those appearing in argument position as well as those appearing in predicate position—he also faced the problem of how to give a compositional semantics for names appearing in argument position. But whereas we proposed that a name appearing in argument position is embedded in a denuded definite description, Burge proposed that a name appearing in argument position is embedded in a denuded complex demonstrative.

Whereas we proposed that 'Maria is tall' was to be interpreted as 'Ø_{the} Maria is tall', Burge proposed that it is to be interpreted as 'Ø_{that} Maria is tall'.[27] Let's call the two views *'The'-Predicativism* and *'That'-Predicativism.*

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[25] So it seems to me. Surely we could devise a more convincing example should need be.

[26] I regard Burge's "appropriateness" condition as equivalent to the being-called condition, even though it is not expressed in terms of the 'called' relation. Bach (1997, 2002) argues for what I consider to be a version of Burge's appropriateness condition. Bach's version is expressed as a "name-bearing" condition: 'Aristotle' applies to a thing just in case it bears the name 'Aristotle'. Geurts also argues for the name-bearing condition. I regard the appropriateness and name-bearing conditions as equivalent to one another (and both to the being-called condition) since—expressed schematically—*having 'N' be a name of yours* and *bearing the name 'N'* are equivalent to being called *N*, even though the name '*N*' occurs in a predicate position in the being-called condition, while it is the name of that name, "'*N*'", that occurs in a predicate position in the appropriateness and name-bearing conditions. To be called *N* is to have *N* as a name of yours. And to be a bearer of the name '*N*' is to be called *N*.

[27] I should mention that Burge did not frame his analysis in quite these terms. Burge never made explicit mention of an *unpronounced* 'that'. Rather, he spoke of names that

The two versions of Predicativism are closely allied, but the definite-description version explains, as we'll see in section 9, certain linguistic facts that make it irresistible and in any case, far superior to its complex-demonstrative cousin.

Burge's own direct argument for 'That'-Predicativism is compressed.[28] Let me quote it in full:

> Evidence for the view that proper names in singular unmodified form involve a demonstrative element emerges when one compares sentences involving such names with sentences involving demonstratives. Apart from speaker-reference or special context, both
>
> Jim is 6 feet tall
>
> and
>
> That book is green
>
> are incompletely interpreted—they lack truth value. The user of the sentences must pick out a particular (for example, a particular Jim or book) if the sentences are to be judged true or false. It is this conventional reliance on extrasentential action or context to pick out a particular which signals the demonstrative element in both sentences. (Burge 1973, 432)

Here Burge points out that sentences involving names—whether or not names are predicates—do not have a truth value independently of context. A brutely applied disquotational truth clause for the sentence 'Jim is six feet tall' is not true:

(40) 'Jim is six feet tall' is true just in case Jim is six feet tall.

That is no more true than this is:

(41) 'I am hungry' is true just in case I am hungry.

Your utterance of the sentence may be true even if I am not hungry. Similarly, if when I utter (40) I am saying anything at all, then in using 'Jim' on the right-hand side I must use it to refer to some particular Jim. But when you utter the sentence mentioned on the left-hand side, if you are to make any claim at all, then you must refer to some particular Jim.

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appear in argument position as "involv[ing] a demonstrative element" (Burge 1973, 432). As Jeffrey King (2006, 148–50) has pointed out, there is more than one way to interpret this claim.

[28] Elugardo (2002) and Dolf Rami (2014) have done an admirable job of extracting and fleshing out an explicit argument for Predicativism from Burge's text.

But they needn't be the same Jim. The Jim I refer to is six feet tall. But the Jim you refer to is less than six feet tall. The truth clause must depend on context:

> (42) An utterance of 'Jim is six feet tall' is true in a context just in case its agent, in making the utterance, refers to some particular Jim and that Jim is six feet tall at the time of the utterance.

By now, we have all learned from Kripke's "semantic argument" that when we do refer to some Jim on an occasion, using the name 'Jim', this cannot always be a result of our having some substantive description of Jim that applies to him and no one else. Jim might well not uniquely satisfy any description that we associate with him. We may refer to him nevertheless.

Nor need we *believe*, Kripke also taught us, that we have some uniquely identifying substantive description of him. I do not believe that I have any substantive *uniquely* identifying description of Cicero the Roman orator. (Whether that is right depends on what notion of "substantive" is in play, though Kripke does not use that word.)

Burge concludes that since we can succeed in saying something about a particular Jim, even though there are many Jims, the particular Jim that we're talking about must have been demonstrated or picked out in some way. Since Burge is concerned to develop a unified semantics of names as predicates, he needs a name appearing in argument position to be embedded within some phrase that can both appear in argument position and have a "demonstrative element"—where the name is the only part that gets pronounced. Complex demonstratives are such expressions, so he plumps for 'that', having already rejected 'the' on the grounds that names typically do not have singleton extensions.

6. Against an Argument against 'That'

Some opponents of Burge's view object to it because they object to the being-called condition.[29] Others object to it because they object to a null 'that'. I'll consider one such argument, a syntactic one due to Jeffrey King (2006). As I'll show, the argument fails because it applies too widely. I consider King's syntactic argument here because it would damage 'The'-Predicativism as much as 'That'-Predicativism if it did not fail.

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[29] See, for example, Stephen Boër (1975).

King's Syntactic Argument

King observes that an overt appearance of 'that' can affect grammaticality. He points to a contrast like that between (43) and (44) and takes this contrast to disconfirm 'That'-Predicativism. He writes:

> The count noun 'dog' [in (44)] cannot be used to designate a particular dog. So here the advocate of the predicate view of names has to hold that names *qua* count nouns exhibit exceptional behavior not exhibited by other count nouns.
>
> (43) John is kind.
> (44) * Dog is kind. (King 2006, 149).

Plainly, names do not exhibit the same behavior as common count nouns. But that is not a strike against 'That'-Predicativism. For consider an analogous argument against the view that mass terms are nouns.

> The mass term 'dust' in (46) cannot be used to designate the kind of thing that can be counted. So the advocate of the view that mass terms are nouns has to hold that mass terms *qua* nouns exhibit exceptional behavior not exhibited by other nouns.
>
> (45) How many dogs do you have?
> (46) * How many dusts do you have?

Why isn't this a good argument against the nounhood of mass terms? Because there *is* more than one kind of noun. There are mass nouns and count nouns, among others. Nouns are not all alike in all respects. Some nouns can be used to count the things that they apply to; others cannot.

Similarly, nouns are not all syntactically alike. A mass term cannot complement the indefinite article. (Witness: 'I vacuumed up a dust this morning'.) A count noun can. If one is to maintain that mass terms are nouns despite their syntactic and semantic differences from count nouns, then one should have an independent criterion for being a noun and show that mass terms satisfy that criterion.

Likewise, if one is to maintain that names are count nouns, then one should have an independent criterion for being a count noun and show that names satisfy that criterion. Things are better if we can also make some diagnostic generalizations about the syntactic differences between names and common count nouns. In the case of mass terms, we can make a number of syntactic generalizations. Mass terms cannot complement the determiners 'a' or 'every'; they cannot occur in the plural; they cannot complement 'how many', but they can complement 'how much'.

We will see that 'the'-predicativists can make correspondingly predictive generalizations about names as compared with common count nouns. We will make good on that claim in section 9.

So Burge has a quality response to King's syntactic argument. King's argument requires that if names are count nouns, they must have the same syntactic properties as common count nouns. We have already seen that this is not so. Common count nouns, such as 'dog' in (44), cannot appear as bare singulars in argument position. Names, such as 'John' in (43), can. This no more tells against names being count nouns than do the syntactic differences between count and mass nouns tell against the thesis that mass nouns are nouns. It does, though, show that names and common count nouns are not in the same syntactic class. So Burge's 'That'-Predicativism survives.

That said, I nevertheless reject 'That'-Predicativism in favor of 'The'-Predicativism. That is because 'The'-Predicativism is better motivated and allows for a more satisfying and cohesive account of the syntactic differences between names and common count nouns.[30]

7. Two Terminological Clarifications

Before proceeding let me make two quick points about my terminology. One concerns the use of the term 'proper name'. The second concerns the use of the term 'common noun'.

Being Proper Is Not an Interesting Property

When philosophers and linguists use the phrase 'proper name', they usually mean a name that denotes an individual of a certain sort, for example, a person, a pet, a country, or a planet. But that list is (obviously) not exhaustive. Names of companies, for example, 'Polaroid', are considered to be proper names. Names of months and days are considered to be proper names, but names of seasons are not. According to the *Chicago Manual of Style* (16th ed., 5.6), "A proper noun is the specific name of a person, place, or thing . . . , or the title of a work." But for the purposes of this definition, what is a "thing"? Names of words, such as the name of the word 'philosophy' ("philosophy"), are not considered to be proper names. But surely words are things. So how do we decide which sorts of things are the ones whose names are proper names?

---

[30] Rami (2014) offers a series of objections to 'That'-Predicativism. Unlike with King's objections, I find Rami's persuasive. But also unlike King's objections, they do not equally well apply to 'The'-Predicativism.

We don't need to try to decide. There is no grammatical difference between names of towns, such as 'Princeton', which are considered to be proper names, and names of words, such as "Princeton", which are not considered to be proper names. I don't believe that there is any interesting distinction between the names that are proper and the names that aren't that can be made apart from the usage that's recommended by style guides. I choose to use the phrase 'proper name' just to mean a name that English-language style guides in general recommend capitalizing. Since for the purposes of this essay capitalization conventions are irrelevant, I eschew usage of the term 'proper name' here.

Names Are Never Used as *Common Nouns*

One of the most dogged criticisms of Predicativism is based on the fact that names and common count nouns are not syntactically alike. Recall King's pair from the previous section, along with its opposite.

(43) John is kind.
(44) * The John is kind.
(47) * Dog is kind.
(48) The dog is kind.

The word 'John' is a name in these sentences. The word 'dog' is a common noun. Both are count nouns. The minimal pairs above demonstrate that although names and common count nouns are both count nouns, they are in different syntactic classes.

The grammatical distributions of names and common count nouns do, however, coincide in many cases. We have seen a number of examples already ('There is one Alfred/university in Princeton', 'Every Sarah/teenager I've met sometimes works as a babysitter', 'Sarahs/Bears from the north are usually scary', and so forth). But I will never say, as some authors do, that in such cases a name is used *as* a common noun in such sentences. As I use the terms, the categories *name* and *common noun* are mutually exclusive. Their grammatical distribution coincides substantially, but not entirely. They differ syntactically in that names but not common count nouns can appear as bare unmodified singulars in argument position.

8. Segal's Challenge

Gabriel Segal made a syntactic argument against 'The'-Predicativism that's just like King's syntactic argument against 'That'-Predicativism.

Segal writes:

> Explicit [determiner[name]] constructions do not function exactly like names with no explicit determiner attached. Witness (49).
>
> (49) a. I live in London.
> b. ?? I live in the/that London.
>
> (49a) provides a perfectly idiomatic response to the question 'where do you live?' posed in an ordinary context... when [the] audience would naturally interpret 'London' as London, England. In the same context, (49b) would sound bizarre.
>
> Contrast (50), uttered in a context in which London, Ontario is not out of the picture.
>
> (50) a. I live in the London that's in S.E. England.
> b. I live in that London (pointing from the window of a suitably positioned aeroplane).
>
> And consider finally (51).
>
> (51) a. This is the/that John I mentioned yesterday.
> b. * This is John I mentioned yesterday.
>
> If there is a hidden determiner in (49a) and (51b), it is evident that there are some special rules governing when it can appear on the surface and when it cannot. ['The'-Predicativism] stands in serious need of a well-motivated account of these rules. (Segal 2001, 560–1)

Indeed.

Segal here has challenged the 'the'-predicativist to explain the data in (49)–(51) in light of her two views that (i) names are a species of count noun and that (ii) bare singular names appearing in argument position complement an unpronounced definite article.[31] To meet this challenge, we must provide explanations of the kind mentioned in response to King's syntactic argument against a null 'that' in the previous section. These were (A) there must be an independent criterion for being a count noun and (B) we should be able to make some predictive generalizations about the syntactic differences between names and other count nouns. We can meet both of these challenges.

---

[31] Segal attributes the argument to James Higginbotham (1988).

(A) For *x* to be used as a count noun is for *x* to be used as a noun that applies to things that can be grammatically counted *using x itself.*[32]

For example, 'dust' cannot be used as a count noun since we cannot grammatically count in this way: one dust, two dusts, three dusts, . . . ; 'cat' can be used as a count noun since we can grammatically count in this way: one cat, two cats, ... Names pass this test easily. Just as we can count the number of cats in my house using the noun 'cat', we can count the number of Johns in my department using the noun 'John': one John, two Johns, ...

(B) We can also make predictive generalizations about the syntactic differences between names and other count nouns. Sloat (1969) has already provided such a generalization. We will provide a slight amendment to Sloat's generalization about the distribution of determiners with names and other count nouns. Then we will formulate a syntactic hypothesis that accounts for it.[33]

9. Why 'The'?

Sloat laid out a chart comparing the distribution of determiners with phrases containing common count nouns with the distribution of those same determiners with a name put in for the common noun. He considers (i) the indefinite article, (ii) 'sm', which is the unstressed 'some' that occurs with plurals and mass nouns, (iii) 'sóme', which is the 'some' that is typically well represented by the existential quantifier, (iv) bare plurals, and (v) the definite article. I have added to his chart some rows for the demonstrative 'that'. Asterisks indicate unacceptable sentences; question marks indicate marginally acceptable sentences.

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[32] The criterion as framed here is use dependent because, as Kathrin Koslicki (1999) has emphasized, that whether a noun is a mass noun or a count noun can be use dependent ('There's hair in my soup' versus 'There's a hair in my soup').

[33] Whereas here we offer a syntactic criterion for when the determiner may be dropped, Sawyer (2010, 217), in defense of 'That'-Predicativism, proposes a semantic criterion. The syntactic criterion is preferable because on the one hand, it applies categorically and on the other hand, there is never any unclarity as to whether it applies in a given case.

The Sloat chart (Sloat 1969, 27)

A man stopped by.	A Smith stopped by.
* Sm man stopped by.	* Sm Smith stopped by.
Sóme man stopped by.	Sóme Smith stopped by.
Sm men stopped by.	Sm Smiths stopped by.
Sóme men stopped by.	Sóme Smiths stopped by.
Men must breathe.	Smiths must breathe.
The clever man stopped by.	The clever Smith stopped by.
The man who is clever stopped by.	The Smith who is clever stopped by.
A clever man stopped by.	A clever Smith stopped by.
The men stopped by.	The Smiths stopped by.
The man stopped by.	* The Smith stopped by.
* Man stopped by.	Smith stopped by.

That clever man stopped by.	That clever Smith stopped by.
?? That man who is clever stopped by.	?? That Smith who is clever stopped by.
* That men stopped by.	* That Smiths stopped by.
That man stopped by.	That Smith stopped by.

Note that 'the' is distinguished as the only one of the determiners that differs in its distribution with names as compared to common count nouns. That is, the following are the only two rows of the table in which there is a difference in acceptability between the left column and the right column.

The man stopped by.	* The Smith stopped by.
* Man stopped by.	Smith stopped by.

The only difference between determiner plus name and determiner plus common count noun is that overt 'the' cannot appear as a determiner before an unmodified name in the singular, unless it is stressed. But the definite article must appear as overt 'the' when it is stressed or when it occurs with a name that is modified or in the plural.

Sloat (1969, 28) proposes that the simplest explanation is that the definite article has a "zero allomorph" (what we have been calling null 'the') and must appear as such before a name in the singular, except when it is stressed or when the name is preceded by a restrictive adjective or followed by a restrictive relative clause, or unless some other determiner appears overtly.[34] These display the relevant cases:

Unmodified singular, no 'the':

(52) I just saw Marc Jacobs.

Stressed article, overt 'the':

(53) I just saw THE Marc Jacobs.

Preceded by a restrictive adjective, overt 'the':

(54) I just saw the famous Marc Jacobs (not my neighbor Marc Jacobs).

Followed by a restrictive relative clause, overt 'the':

(55) I just saw the Marc Jacobs who designs those beautiful handbags (not the Marc Jacobs who works at Google).

In the next section I will argue that Sloat's descriptive generalization is not quite right. My aim is to defend 'The'-Predicativism against Segal's criticism from section 9 by actually giving a "well-motivated account" of the "special rules" that govern when the hidden determiner "can appear on the surface and when it cannot." The main reason to prefer 'The'-Predicativism to 'That'-Predicativism is that the account of the "special rules" in question is well motivated when the hidden determiner is $\emptyset_{the}$, but not when it is $\emptyset_{that}$. The main reason to amend Sloat's generalization is that doing so enables us to give a very simple generalization to account for the distribution of $\emptyset_{the}$ to replace his disjunctive one.

10. Amendments to Sloat's Generalization

Sloat described three cases where 'the' would appear overtly in a definite description containing a singular name. The first concerned stressed 'the'. The second and third concerned restrictive modifiers: singular names preceded by restrictive adjectives or followed by restrictive relative clauses would require the definite article to appear as overt 'the'. My aim in this section is to argue for certain amendments to Sloat's descriptive generalization. I have found that if we make the amendments that I propose, then we can derive the distribution facts about $\varnothing_{the}$ from a certain very simple syntactic rule.[35]

---

[34] This is a fairly direct paraphrase of Sloat (1969, 27).

My first amendment to Sloat's generalization concerns his reference to modifiers of the specific grammatical categories *adjective* and *relative clause.* There are restrictive modifiers of other categories that force the definite article to appear overtly with singular names. For example:

(56) The designer Marc Jacobs is opening a new store.
(57) The Marc Jacobs in my kitchen is dicing habañeros.

The restrictive modifier 'designer' in (56) could be called an attributive noun. It is not an adjective, but it does require the definite article to be overt. In (57), the restrictive modifier 'in my kitchen' is not a relative clause but rather a prepositional phrase. But it also forces the definite article to be overt. So let's be more general than Sloat was by saying that it is restrictive modifiers coming before or after a singular name that force overt 'the', regardless of their grammatical category.

My second amendment concerns *non*restrictive modifiers. While Sloat thought that restrictive modifiers before a name required an overt definite article, he thought that *non*restrictive modifiers before a singular name required a null definite article. He came to this conclusion on the basis of the following contrast:

(58) I talked to young Martin about it.
(59) I talked to the young Martin about it.

He judges that without overt 'the', as in (58), 'young' is unambiguously nonrestrictive—like 'brave' is when we say, "Brave Philip slew the dragon." But Sloat judges the opposite with (59). He judges that 'young' in (59) is unambiguously restrictive—like 'younger' is when we say "The younger Philip slew the dragon." Sloat infers that when a modifier occurs before a singular name in a definite description, a restrictive interpretation requires overt 'the' while a nonrestrictive interpretation prohibits overt 'the'.

I agree with Sloat's judgment about (58): it does have only the nonrestrictive reading. I disagree with Sloat's judgment about (59), however. I think that the sentence is ambiguous: the modifier 'young' can be used nonrestrictively as well as restrictively. I might use 'young' restrictively in (59), to winnow out the nonyoung Martins from the set of Martins containing the one I want to speak about. But I might also use it nonrestrictively, to attribute youth to the Martin that I'm speaking about while saying about him that I talked to him. On this point, I agree with Matushansky (2006b, section 2.1), as against Sloat, but I want to provide a more extended discussion of, and argument for, the point.

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[35] To my knowledge, Matushansky was the first (and only) person to propose a simple syntactic rule from which Sloat's disjunctive generalization could be derived. The rule I propose differs from Matushansky's; it's simpler in my opinion, and it accounts for more cases.

There is a straightforward test for the availability of a nonrestrictive interpretation for a given modifier.

Nonrestrictive-Interpretability Test:

> If someone who believes that there is only one *F* can sincerely utter a sentence containing the phrase '*M F*', where *M* is a modifier, then the modifier *M* can have a nonrestrictive interpretation in that sentence.

The modifier 'young' in (59) passes this test. As long as I do not pronounce (59) with any kind of contrastive intonation, I can sincerely utter it even if I believe that there is only one Martin. I envisage a situation like this: I want to draw attention to Martin's youth because the thing that I talked to him about was a matter involving social subtlety and nuanced emotions, not something easily comprehended by the young. This comes across more easily if we add 'even'.

(60) I was at such a loss what to do, I even talked to the young Martin about it.

There is nothing incoherent in my saying this while believing that there is only one Martin.

Recall that Sloat's combined judgments about (58) and (59) led him to the following generalization.

Sloat's generalization about prename modifiers:

> When a singular name is preceded by a modifier, the definite article must appear (i) as null if the modifier is used nonrestrictively, but (ii) as overt 'the' if the modifier is used restrictively.

My dissenting judgment about (59)—that it does have a nonrestrictive reading—leads me to go in for an alternative generalization.

My alternative generalization about prename modifiers:

> When a singular name is preceded by a modifier, the definite article must appear as overt 'the' whether the modifier is used restrictively or nonrestrictively.

Both generalizations have counterexamples. We will consider them in turn. Keep in mind, though, that a generalization needn't be rejected entirely just when it has some exceptions. The exceptions might themselves be unifiable in accordance with some principle. The hope would be to hold on to one of the generalizations and countenance the exceptions to it while looking for a supplementary generalization to unify the exceptions. We cannot hold on to both generalizations since they are incompatible with one another. I will argue shortly that Sloat's generalization is the one that has to go. So let's now go through the counterexamples to each of the generalizations.

Sloat's generalization has counterexamples since in at least some sentences, modifiers that occur between a name and overt 'the' do have nonrestrictive readings in addition to their restrictive readings. Any such sentence constitutes an exception to Sloat's generalization about prename modifiers. Let me give some examples:

(61) The talented Mr. Ripley was a terrific impersonator.
(62) The ever-popular Marc Jacobs won yet another design award.

The prename modifier in (61) *could* be used restrictively to contrast the talented Mr. Ripley with some different, untalented Mr. Ripley that's a terrible impersonator. But it could also be used nonrestrictively, to attribute talent to the particular Mr. Ripley that's being spoken about—even if it is known that there is just one Mr. Ripley in existence. Likewise with (62). I *might* use the prename modifier restrictively because there is some unpopular Marc Jacobs who's never won an award of any kind. But I might also use it nonrestrictively. It might be that there is just one Marc Jacobs and that I want to draw attention to his enduring popularity while saying about him that he won yet another design award.

Let's avoid the difficult question of what the truth-conditional contribution of nonrestrictive modifiers is and note merely that (59)–(62) are all exceptions to Sloat's generalization about modifiers occurring between a singular name and overt 'the'. In all of these sentences, the modifiers can be interpreted nonrestrictively as well as restrictively.

Meanwhile, there are also clear exceptions to the alternative generalization about prename modifiers. We have already seen such an exception in Sloat's example (58), 'I talked to young Martin about it', in which a nonrestrictive modifier precedes a singular name but with no overt 'the' (or any other determiner). And there are many modifiers that are like 'young' in this respect. We can call them *null-permissive modifiers.* Null-permissive modifiers permit $\varnothing_{the}$ when occurring before a singular name while being interpreted nonrestrictively. The following phrases illustrate that there are many null-permissive modifiers, witness: *simple Simon*; *poor Aunt Beth*; *lucky Pierre*; *good old John*; *fearless Evel Knievel*; *sweet Georgia Brown*; and *sharp-eyed Éamonn McManus.*[36]

It seems to me that 'talented' is a null-permissive modifier but that 'ever-popular' is not.

(61′) Talented Mr. Ripley was a terrific impersonator.
(62′) * Ever-popular Marc Jacobs won yet another design award.[37]

And there are other cases like (62′)—cases in which the occurrence of a modifier before a singular name requires the definite article to be overt, and yet can be interpreted nonrestrictively as well as restrictively.

(63) a. The dying John Smith asked to see his ex-wife.
b. * Dying John Smith asked to see his ex-wife.
(64) a. The former Mrs. Smith was ecstatic about her divorce.
b. * Former Mrs. Smith was ecstatic about her divorce.[38]

We can use the phrase *strict modifier* to classify modifiers like these—ones which prohibit $\varnothing_{the}$ when occurring before a singular name while nevertheless being used nonrestrictively.

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[36] I borrow these examples from Geoffrey Pullum (2006).

[37] If we pronounce the sentence with "comma intonation," with a pause after 'ever-popular', then it is fine. But in that case, the adjective is not occurring as a modifier of the name, but rather as a preposed appositive phrase, yielding an equivalent to 'Marc Jacobs, ever popular, won yet another design award'.

[38] Since 'former' is not an intersective modifier, it is somewhat strange to think of it as being used either restrictively or nonrestrictively. One would think that for a modifier M to be used restrictively when combined with a noun F is for the phrase 'MF' to denote the intersection of the Ms and the Fs. But then by that definition, nonintersective modifiers could never be used restrictively. Nevertheless, I think that the distinction between restrictive and nonrestrictive is sufficiently clear in the case of nonintersective adjectives for us not to worry about the oddity of the terminology. There might be a current Mrs. Smith that I want to contrast the former Mrs. Smith with (restrictive). Or I might be making no contrast at all: there may be no current Mrs. Smith and no former Mrs. Smith other than the one being talked about (nonrestrictive).

We now have these categories:

Null-permissive modifiers:

> Ones that permit $\emptyset_{the}$ when occurring before a singular name.

Strict modifiers:

> Ones that prohibit $\emptyset_{the}$ when occurring before a singular name while simultaneously being interpreted nonrestrictively.

And the situation we are in is this: Strict modifiers, such as 'ever-popular', 'former', and 'dying', provide counterexamples to *Sloat's Generalization about Prename Modifiers,* as stated above. Meanwhile, null-permissive modifiers, such as 'young' and 'brave', provide counterexamples to the *Alternative Generalization about Prename Modifiers,* as stated above. If we want to provide a well-motivated account of the rules that govern when the definite article can appear on the surface and when it cannot, then we should look for simple generalizations. One option would be to jettison both of the above generalizations and cast about for a replacement. But since we seem to be on the right track, it would be better to hold on to one of the generalizations and try to unify the exceptions according to some supplementary principle.

If we do that, then the *Alternative generalization* is the one that stays. To explain why, it helps to introduce a third category of modifiers, which can be called *'the'-permissive.*

'The'-permissive modifiers:

> Ones that permit the presence of overt 'the' when occurring before a singular name while still being interpreted nonrestrictively.

The following sentences display that 'brave', 'young', and 'aging' are 'the'-permissive:

(65) The brave William never shed a tear.
(66) The young Harry followed in his brother's footsteps.
(67) The aging Elizabeth retained her stiff upper lip throughout all of the scandals.

Note that 'aging' is like 'dying' in that it cannot occur with $\emptyset_{the}$. That is to say that both are strict modifiers.

(68) * Aging Elizabeth retained her stiff upper lip throughout all of the scandals.

The point of disagreement, then, between Sloat's generalization and the alternative one boils down to this: according to Sloat's generalization, there are no 'the'-permissive modifiers; according to the alternative, there are no null-permissive modifiers. The reason to reject Sloat's generalization in favor of the alternative is that *every* null-permissive modifier is also 'the'-permissive. Or so I claim. If true, the claim gives us a reason to reject Sloat's generalization about prename modifiers.

Claim:

> Every modifier that can occur before a singular name with $\emptyset_{the}$ can also occur with overt 'the' while still being interpreted nonrestrictively.

It is difficult to prove a universal when there is no such thing as an arbitrary instance, so I will not try to conclusively demonstrate my claim that every null-permissive modifier is also 'the'-permissive. But let me lend it plausibility by reviewing that every null-permissive modifier that we have looked at so far is also 'the'-permissive.

Null-permissive modifiers:

(69) Young Martin would be happy to stay young forever.
(70) Brave Philip slew the dragon with just one blow.
(71) Sharp-eyed Éamonn McManus noticed the mistake right away. (Pullum 2006)

'The'-permissive modifiers:

(72) The young Martin would be happy to stay young forever.
(73) The brave Philip slew the dragon with just one blow.
(74) The sharp-eyed Éamonn McManus noticed the mistake right away. (Pullum 2006)
(75) The inimitable Josep wowed the audience with his virtuosity.
(76) The simmering Bugs Bunny taunted his captor by telling jokes while the broth boiled.
(77) The infuriated Yosemite Sam jumped up and down in anger.

Strict modifiers:

(78) * Inimitable Josep wowed the audience with his virtuosity.
(79) * Simmering Bugs Bunny taunted his captor by telling jokes as the broth heated up.
(80) * Infuriated Yosemite Sam jumped up and down in anger.

Now since, as I claim, every null-permissive modifier is also 'the'-permissive, every modifier whatsoever constitutes an exception to Sloat's generalization. But the exceptions to the alternative generalization are the modifiers that are null-permissive. Since there are modifiers that are not null-permissive but that can be used nonrestrictively—that is, there are modifiers that are strict—the alternative generalization has but a limited class of exceptions. The hope would be that the limited class of null-permissive modifiers could be unified according to a supplementary general principle.

In the next section I explain how once we incorporate my two amendments, Sloat's descriptive, disjunctive generalization concerning the null definite article can be distilled into one simple principle.

11. Where 'The' Must Be Null

The proposal now on the table is that names *always* occur in predicate position. When one appears not to be in predicate position, it is because it is embedded in a denuded definite description. The rule governing the stripping of overt 'the', as garnered from the last section, is this: The definite article appears as $\varnothing_{the}$ when it occurs with a name, except in any of the following cases, where it appears overtly as 'the':

(i) The definite article is stressed.
(ii) The name is preceded by a restrictive or nonrestrictive modifier.[39]
(iii) The name is followed by a restrictive modifier.

Sloat's 'adjective' and 'relative clause' have here been replaced with the more inclusive term 'modifier'.

The question now arises whether we can dispense with our disjunctive version of the generalization concerning $\varnothing_{the}$ in English and provide a simpler generalization of the rule. It turns out that one clearly emerges when we look at the syntactic structures associated with the cases subsumed under the rule. First look at cases where the definite article is required to be null.

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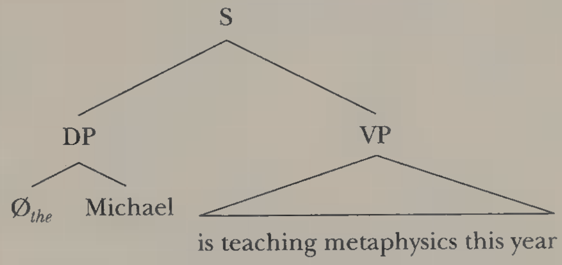
[39] We should keep in mind that null-permissive modifiers are exceptions to the part of this rule that governs nonrestrictive modifiers. I am optimistic that some tidy way of circumscribing the null-permissive modifiers will be discovered.

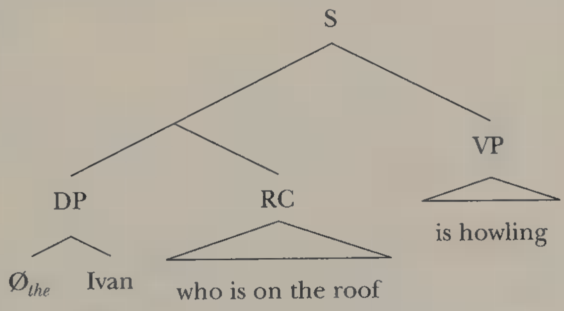
The Basic Case:

(81) Michael is teaching metaphysics this year.



The Name Is Followed by a Nonrestrictive Modifier (here a relative clause):

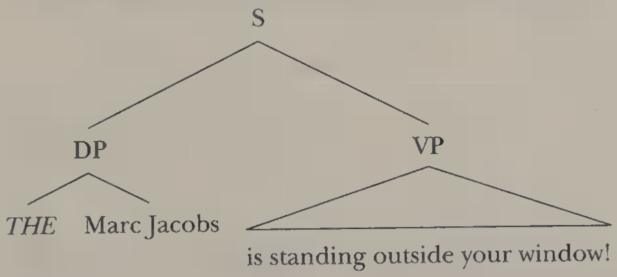
(82) Ivan, who is on the roof, is howling.
(I don't know why he's on the roof.)



Next look at structures where the definite article is required to be overt.

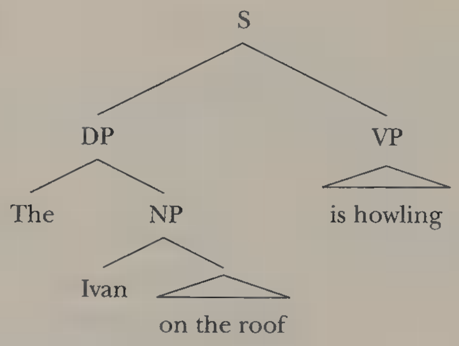
The Definite Article Is Stressed

(83) *THE* Marc Jacobs is standing outside your window!
(You mean it's not my neighbor Marc Jacobs?!)



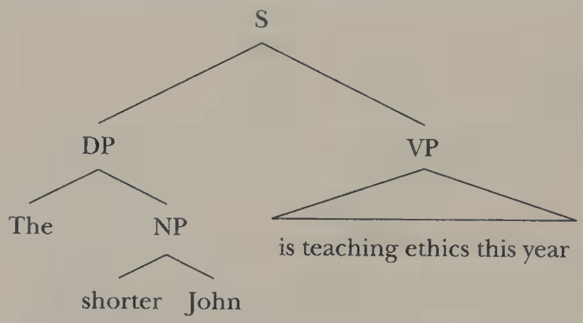
The Name Is Followed by a Restrictive Modifier (here a prepositional phrase):

(84) The Ivan on the roof is howling.
(But the Ivan downstairs is passed out.)



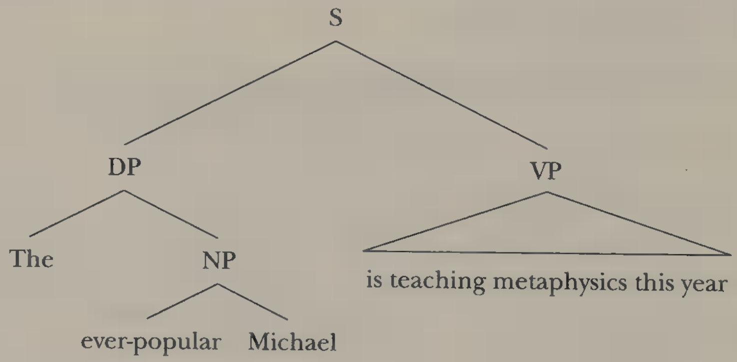
The Name Is Preceded by a Restrictive Modifier

(85) The shorter John is teaching ethics this year.
(The taller one is teaching logic.)



The Name Is Preceded by a Nonrestrictive Modifier

(86) The ever-popular Michael is teaching metaphysics this year.
(His clarity and kindness are what make him so popular.)



There is a general principle that covers all cases where the definite article must be realized as $\varnothing_{the}$, namely, whenever it has a name as its sister, unless it is stressed. The 'the'-predicativist has a straightforward explanation for why the definite article is pronounced when stressed in cases where it would otherwise not be grammatical: one cannot stress a word without pronouncing it!

Where $\varnothing_{the}$: The definite article must appear as $\varnothing_{the}$ when it has a name as its sister, unless it is stressed.[40]

One could think of this as a kind of obligatory contraction: when nothing occurs "between" the definite article and a name, they get smushed together, resulting in an unpronounced definite article (discounting those cases in which the article is stressed for emphasis). One way for a word or phrase to get between the definite article and a name is to be linearly between them, as in (62), 'The ever-popular Michael is teaching metaphysics this year'. But linear adjacency of the definite article and the singular name is not a sufficient condition for contraction. A branching node that dominates the name but not the definite article also gets "between" them in the sense that's relevant for blocking contraction, as in (84), '[The [Ivan [on the roof]]] is howling'.

So what, now, is our analysis of the difference between the interaction of names with the definite article and that of common count nouns with the definite article?

The man stopped by.	* The Smith stopped by.
* Man stopped by.	Smith stopped by.

The difference displayed here can be derived from the following principles.

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[40] Matushansky (2006a, 2006b) accounts for the appearance of overt 'the' with names by proposing a particular rule of "head movement," which she dubs "m-merger," in which a name in a definite description raises and adjoins to the definite article if it is close enough to it, in a certain well-defined sense. The name and the article in that case "merge" morphologically. In English, she proposes, this morphological merger leads to an unpronounced definite article. In contrast, in Catalan (among other languages), m-merger leads to the definite article's being pronounced differently than it would be otherwise, as a special "preproprial definite article" (Matushansky 2006b, sec. 4). My objection to Matushansky's specific proposal is that it doesn't account for the fact that the definite article cannot appear as null when a name is followed by a restrictive modifier, as in our case above, 'The Ivan on the roof is howling', since in that case the name and the definite article are close enough to each other, in the specified sense. For her proposal to be adequate, the prepositional phrase 'on the roof' must intervene between 'the' and 'Ivan' at the level at which m-merger takes place. My proposal does not depend on there being any particular kind of movement.

1. Singular count nouns cannot occur bare in an argument position, but when they appear to do so they occupy the predicate position of a denuded definite description. (section 4)
2. Names are count nouns. (section 1)
3. The definite article must be null just when it is unstressed or has a singular name as its sister. (section 11)

12. The Import of the Syntactic Evidence for $\emptyset_{the}$

Let me sum up the arguments and conclusions from the preceding four sections (sections 8–11). The main point to be highlighted is this. The existence of the null definite article has been confirmed by a number of syntactic facts that are entirely independent of concerns for a unified compositional semantics for names.

Sloat's chart, which displays how names and common count nouns interact with overt determiners, demonstrates that not only can the overt definite article show up with singular names, it is completely predictable which environments do not tolerate an overt definite article and which ones require a definite article to appear overtly. The definite article must appear overtly with singular names when it stressed, when the name is preceded by a restrictive or nonrestrictive modifier (but for some exceptions discussed in section 10), or when the name is followed by a restrictive modifier; otherwise it is required to be null. Segal (2001, 561) had noted that if 'The'-Predicativism were to be plausible, it needed a "well-motivated account" of the "special rules" governing when the definite article "can appear on the surface and when it cannot." Sloat's chart, in virtue of its well-organized and comprehensive presentation, cleared the way for the aforementioned disjunctive description of these special rules. This all satisfies Segal's requirement that there be an *account* of the "special rules" in question.

Furthermore, with my two amendments to Sloat's descriptive generalization, for which I gave extended arguments in section 10, I was able to formulate a very simple syntactic rule to capture the generalization and so replace Sloat's disjunctive characterization with a far simpler one, in section 11:

Where $\emptyset_{the}$: The definite article must appear as $\emptyset_{the}$ when it has a name as its sister, unless it is stressed.

Here I leaned heavily on the work of Matushansky (2006b), but proposed a different, and simpler, syntactic rule from the one that she proposed, which she calls *m-merger* (2006a, 2006b). Note that we find further evidence for the existence of a null definite article in the facts that (i) it may always be pronounced when it is stressed (you cannot stress a word without pronouncing it) and (ii) a number of languages permit pronunciation of the definite article as it occurs with unmodified singular names. Matushansky (2006b, 2008) provides an excellent discussion of the cross-linguistic facts. All of this satisfies Segal's further requirement that the account of the "special rules" be *well motivated.*

Moreover, the proposed account of the distribution of overt 'the' with singular names cannot be appropriated either by referentialists or even by 'that'-predicativists.

The account cannot be appropriated by referentialists since it posits that when a singular name appears bare (without a determiner), it is really embedded in a denuded definite description. Thus if the solution is right, then names *never* occur in an argument position, only ever in a predicate position.

And the account cannot be appropriated by 'that'-predicativists since accounting for Sloat's distribution patterns requires that the null determiner occurring with apparently bare singular names be the null definite article; a null 'that' or 'this' provides no explanation for the occasional appearance of 'the' with singular names.

So not only does Predicativism provide for a unified semantics for names, on its best version, 'The'-Predicativism, it provides for a simple explanation of what on the referentialist or 'that'-predicativist view is a thoroughly puzzling fact: the regular and predictable distribution of the overt definite article with singular names. The denuded definite descriptions theory of apparently bare, unmodified singular names is not merely semantically motivated, but also explanatorily powerful from a syntactic point of view. And in that respect, it proves superior to its main rivals.

13. Rigidity and Incompleteness? Not Problems for 'The'-Predicativism

Kripke taught us that names are rigid designators, at least in the very (!) restricted class of cases when they appear as bare unmodified singulars in argument position. A name is "a *rigid designator* if in every possible world it designates the same object" (Kripke 1980 [1972], 48). Kripke was concerned to argue against a view he attributed to Frege—that (i) the sense of a name is some substantial description believed by users of the name to apply uniquely to its referent, and (ii) the referent of a name is whatever object uniquely satisfies that description. Kripke took it to be a consequence of this view that if a name were to be replaced in a sentence with the description that gave it its sense, then the two sentences would have the same modal profile (be true in just the same possible worlds).[41]

Kripke's point was that if the truth-conditional contribution of a name were identified with that of some substantive description believed by speakers to hold of the name's referent, then if that description didn't express an essential property of that referent, then names would not be rigid designators. For example, if the sense of the name 'Aristotle' could be expressed as *the last great philosopher of antiquity*, then according to Kripke's Frege, the sentence 'Aristotle was illiterate' would have the same truth conditions as the sentence 'The last great philosopher of antiquity was illiterate'. But these sentences do not have the same truth conditions. If Aristotle had been born into slavery and so never had become literate, and Plato had been the last great philosopher of antiquity, then the sentence 'Aristotle was illiterate' would be true while the sentence 'The last great philosopher of antiquity was illiterate' would be false.

Meanwhile, Kripke convinced us that the descriptions that name users associate with names don't typically apply essentially to individuals (and often do not even apply uniquely to any individual at all). Thus Frege's theory of sense, given Kripke's interpretation of it, is wrong.

This argument against Frege's theory of sense has come to be called *the modal argument*: if the sense of a name were identified with that of some substantive description that applied only contingently to the referent of that name, then sentences in which the name occurred would not have the truth conditions that they in fact have. The import of this issue for us is that since *being called Aristotle* is an accidental property of Aristotle's (he might have been called Alcibiador), we should worry that the description 'the *x* such that *x* is called Aristotle' does not rigidly designate Aristotle. Therefore, this argument against 'The'-Predicativism would go, since 'Aristotle' is a rigid designator, it cannot make the same semantic contribution to sentences as does the description 'the *x* such that *x* is called Aristotle'.

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[41] I am going to stick to the old-fashioned practice of reserving the preposition 'in' for worlds and contexts ('true in a world, in a context', 'designates *o* in a world, in a context', and so forth) and the preposition 'at' for times ('true at a time', 'designates at a time', and so forth).

Rigidity and the Being-Called Condition

So 'the'-predicativists who agree with Kripke's view about the truth conditions of modal claims involving names have to reconcile the following theses:

a. 'Aristotle' is a multiply applicable predicate that applies contingently to a thing just in case it is called Aristotle.
b. When 'Aristotle' appears as a bare singular in argument position, the name occurs as the sister of an unpronounced definite article $\emptyset_{the}$.
c. The unpronounced definite article has the same meaning as the definite article that is overtly realized as 'the'.
d. When the name 'Aristotle' appears as a bare singular in argument position, it functions as a rigid designator in Kripke's sense.

It follows from the first three of these that when the name 'Aristotle' appears as a bare singular in an argument position, the sentence containing it is semantically the same as one containing the incomplete definite description 'the Aristotle' put in place of the name 'Aristotle'.

Together with the fourth of these theses, it follows that the incomplete definite description 'The Aristotle' is a rigid designator. This is precisely what 'the'-predicativists must maintain. But can this be maintained without being ad hoc? The answer is "yes." Incomplete definite descriptions in general are rigid designators. For convenience, we will call this *the rigidity thesis.*

Incomplete Definite Descriptions Are Rigid Designators

Begin by considering a sentence with the incomplete description 'the party':

(87) Olga enjoyed the party.

We will shortly contrast this with a modal variant:

(88) Olga might have enjoyed the party.

If you were to sincerely utter (87), you would be talking about some one particular party—call it BASH—and saying of *it* that Olga enjoyed it. Suppose that the utterance is true: that Olga did in fact enjoy BASH. Someone who overhears your utterance of (87) might well not know what party you are talking about. If he asks you, "what party are you talking about?" you might say, "Naomi's fortieth birthday party." Or you might equally satisfyingly respond by saying "the party I went to last night." Since your aim is to enable your audience to know which of the many parties that happened last night is the one that you are talking about, it is appropriate to give any answer that will adequately convey to your audience which party it is that you are talking about. Nothing in such exchanges requires that there be some one particular completing description that enters into the proposition expressed by your original utterance of (87).[42] When you use the incomplete description 'the party', you use it to talk about a particular party, to say something about *it*—as opposed to whatever party is the so-and-so (as Donnellan 1966 might have put it), where "is the so-and-so" is supposed to stand in for some particular completing description recovered from the context.

According to Russell's theory of descriptions (1905, 1919), a simple sentence such as (87) will be true only if the predicate in its definite description, 'party', applies to just one event. But there are always many parties going on; the utterance is nevertheless true. Looked at the other way: since there is always more than one party going on, a brute application of Russell's theory yields that (87) is false, even though BASH, the party being talked about, was in fact enjoyed by Olga.

Those who wish to maintain Russell's theory of descriptions, despite the rampant use of incomplete descriptions, have four choices. One: cling to the falsity of the envisaged utterance of (87) and explain why we think it's true. Two: accept that utterance as true and explain how the incomplete description is in fact a complete one. Three: give up bivalence for meaningful utterances. Four: say that the utterance is not meaningful.

Those in the second camp, "the contextual completers," must provide that the incomplete definite description 'the party' is associated with a complete definite description—the one to which Russell's theory is to apply. An example could be 'the party I went to last night'. The association with that complete description must be tight enough so that applying the theory to the complete description just is applying it to the original description, which must therefore have merely appeared to be incomplete.

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[42] On this point, I agree with Kripke (1977) and David Lewis (1979 [1983]) and disagree with Nathan Salmon (1982), Stephen Neale (1990), and Jason Stanley and Zoltán Szabó (2000).

Let's suppose (for the sake of argument) that the complete description to be associated with 'the party' is the complete one just singled out, namely, 'the party I went to last night'. Two different types of association have been proposed.

The first is *ellipsis*: one uses the incomplete description just as shorthand for the complete description (in a sense of "shorthand" that needs explaining); the sentence uttered is the very same one that would have been uttered if the speaker had spelled everything out.[43]

The second type of association is by *contextual completion*. One way to implement this idea would be to say that the noun 'party' has a restrictive variable syntactically attached to it, representable as 'party$_F$'.[44] In this case the variable '*F*' would be contextually assigned a property to be intersected with the property of being a party. In our case, '*F*' could be assigned the property of *being an event that I went to last night.*

Yet a third sort of treatment would be to render the definite description complete in the context in which it's uttered by allowing the sentence to be evaluated in a restricted domain—*the domain of the context.* If the things being quantified over are restricted so as to exclude all parties except for any that I went to last night, then the description "the party" will be complete *in its context,* since I did not go to more than one party last night. I would not consider this proposal further, however; it is not viable because it cannot accommodate mixed-domain sentences like these:

(89) The dog got in a fight with another dog (McCawley 1979).
(90) Everyone is asleep and is being monitored by a research assistant (Soames 1986).
(91) Nobody cared that nobody came (Stanley and Williamson 1995).

But now return to our modal variant.

(88) Olga might have enjoyed the party.

According to both the ellipsis and contextual-completion proposals, that sentence would have the same truth conditions as this one:

(88′) Olga might have enjoyed the party that I went to last night.

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[43] See Wilfrid Sellars (1954) for support of the ellipsis proposal. But also see Strawson (1954) and Stanley (2002a) for dissent.

[44] See Stanley and Szabó (2000) and Stanley (2002b) for this sort of treatment.

Given the standard view that the definite description can take narrow as well as wide scope with respect to the modal 'might', (88$'$) would be ambiguous because it would have two scope readings:

(88$'_N$) This might have been: Olga enjoyed the party that I went to last night.

(88$'_W$) The party that I went to last night is an *x* such that this might have been: Olga enjoyed *x*.

But (88) does not have both of these scope readings. It only has the wide-scope reading represented in (88$'_W$).

The original sentence, (88), is true just in case there is a possible world in which Olga enjoyed BASH, whether or not I went to BASH in that world. It is irrelevant that there are possible worlds in which I went to some *different* party last night that was enjoyed by Olga. In other words, (88) has only the truth conditions that it would have if the incomplete definite description were rigid. Since incomplete definite descriptions such as 'the table' or 'the party' can be used to talk about different tables or parties on different occasions, it is right to say that these definite descriptions are *rigid-in-a-context.*

The same holds for other modal sentences like (88):

(92) The party might have been fun.
(True if there is a possible world in which BASH was fun—whether or not I went to BASH in that world.)

(93) More people might have gone to the party.
(True if there is a possible world in which more people went to BASH than in fact went—whether or not I went to BASH in that world.)

(94) I might not have gone to the party.
(True if there is a possible world in which I did not go to BASH, even if I would have gone to some other party had I not gone to BASH.)

(95) If Noah's birthday had been yesterday, then I wouldn't have gone to the party.
(True since if Noah's birthday had been yesterday, then I wouldn't have gone to BASH but rather to Noah's party since I prefer Saturday-night parties to Sunday-night ones.)

So in the face of incomplete definite descriptions, proponents of Russell's theory face a problem: how to provide that incomplete descriptions are made complete by the point at which Russell's analysis applies. Moreover, it is not just Russellians who face this problem. Russellians face the problem as a potential counterexample to their theory. But all theorists of descriptions face the problem as data to be accommodated by their theory.

I said in section 4 that I had no intention of providing an analysis of every construction in which names occur. For example, I said that I would not venture an analysis of the bare-plural constructions in which names and common count nouns both occur. Rather, I confined myself to the claim that whatever the right analysis is of the bare-plural construction in (34a) it is also the right analysis of (34b). I called this piggybacking.

(34) a. Bears from the north are usually scary.
    b. Sarahs from $\emptyset_{the}$ Alaska are usually scary.

Likewise, whatever the right analysis is of donkey anaphora with a common noun, as in (38a), it is also the right analysis of donkey anaphora with a name (38b).

(38) a. Every woman who owns a donkey beats it.
    b. Every woman who knows an Agatha loves her.

Piggybacking allows 'the'-predicativists to develop a theory of names without having to theorize about every problematic construction in which a name occurs. Piggybacking allows us to proffer a theory that can be assessed even while we don't have a comprehensive theory of every problematic construction. Piggybacking can make work easier.

But is there really license to piggyback here, in the case of the rigidity of incomplete descriptions? We are granted such a license only if denuded definite descriptions with names are in fact incomplete in the relevant sense. But while most names have more than one bearer, this needn't have been true. And who knows? It might well not be true tomorrow. Suppose the name 'Kivlighan' has exactly one bearer, then '$\emptyset_{the}$ Kivlighan' is *not* an incomplete definite description in the usual sense. So does this mean that some denuded descriptions with names cannot be argued to be rigid in the present way?

I say no since the situation is the same with books and tables. I say that the definite descriptions in (96) are both rigid.

(96) The book is on the table.

Here's an example to illustrate the point. Suppose I've been looking around for the book that I've been reading. You utter (96) in order to let me know that the book is on the table. Suppose I then wonder whether the book would have been on the table if there had been a tornado, say. What I want to know is whether *b* might not have been on *t* if there had been a tornado, where *b* is the particular copy of *Herzog* that I was looking for and *t* is the particular table that you informed me it was on. It's irrelevant that I would have been reading some different book if there had been a tornado, say, *The Wonderful Wizard of Oz.* And the following is true, but also irrelevant: if there had been a tornado, not only would I have been reading *The Wonderful Wizard of Oz,* I would have been looking for *it,* having absent-mindedly left *it* on the table. In that case this is *true*:

(97) If there had been a tornado, then the book I would have been reading would have been on the table.

But as long as the tornado world is not one in which *b* is on *t,* this is true:

(98) If there had been a tornado, then the book would not have been on the table.

The reason (98) is true is that if there had been a tornado, *Herzog* (my copy of it) would not have been on the table, it would have been on the shelf since I'd have been reading a different book.[45] Meanwhile, if there had been a tornado, there would be a book on a table. And it would have been one that I was both reading and looking for. But still, for all that, (98) is true.

But nothing in what I've just said presupposes that there are other books in the world. While we do tend to assume that there are many tables and books in the world, if there are any at all, our utterances would not have a different meaning if that assumption about the world turned out to be wrong. If incomplete definite descriptions *as such* were grammatically different from complete ones *as such* then, as Christopher Gauker (1997) has forcefully argued, the grammar of 'the book is on the table' would change between today and tomorrow if every book other than the ones being talked about today were destroyed in the night. But it does not. The incomplete descriptions in 'the book is on the table' would be rigid even if, incredibly but unbeknownst to the speaker, there were no books or tables other than the ones he was talking about.

The upshot is that there is no onus on me to provide an explanation of how or why incomplete descriptions are rigid designators. That is the right of the piggybacker. I don't have to take a stand, for example, on what the mechanism is that renders incomplete definite descriptions rigid—on whether, for example, rigidity requires a covert 'actually' operator or whether, alternatively, there is more than one definite article for a speaker to use, one that's more Russell-like and one that's more demonstrative-like, according as it matters to the speaker whether his descriptive material applies uniquely or not.[46] I may content myself with the claim that whatever the semantics for incomplete definite descriptions is, and whatever the mechanism is that renders them rigid, denuded incomplete definite descriptions with names will follow suit.

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[45] I'm leaving out explicit relativization to utterances of the sentence and contexts of utterances, which I would put in if I were being more precise.

That said, there are many complications in the rigidity thesis that incomplete definite descriptions are rigid designators. I won't seriously investigate these here, but will instead just make some gestures regarding three of them.

Relational Predicates: Some definite descriptions containing relational predicates such as 'the murderer', 'the president', 'the mother' appear to be incomplete since there are many murderers, presidents, and mothers in the world. I would propose that these definite descriptions merely appear to be incomplete. As relational predicates, they have a hidden contextual variable: [murderer OF x], [president OF y], [mother OF z]. Since each variable is assigned a value in context, the definite descriptions in question are in fact complete, relative to a cooperating context.

Role-Type Incomplete Definite Descriptions: Kent Bach (2004) has pointed out that incomplete definite descriptions are not always used to refer, and therefore not always used to refer rigidly. Here are two of his examples.[47]

(99) The rental car should have four doors.
(100) The party should have excellent wine.

In neither sentence are we using the incomplete definite description to talk about any particular thing. Rather, we are stating a condition that any rental car, or party, must meet if it is to be the one that we decide to rent, or to go to.

Borrowing a helpful term from Daniel Rothschild (2007), we can call these *role-type* incomplete descriptions. Rothschild makes a distinction between role-type descriptions and "particularized" ones. Role-type descriptions are those for which it is presupposed that they will have just one object that satisfies them, if any at all. To satisfy the description is to be the one to play a certain *role.*

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[46] Larson and Segal (1995) consider this latter idea.

[47] These examples were communicated to me personally.

Rothschild's distinction is pragmatic insofar as whether a description is role-type or not can vary depending on what presuppositions are in force in a conversation—that is, what propositions its participants are jointly taking for granted for the purposes of that conversation.

The reason that 'the rental car' and 'the party' can be used nonrigidly in (99) and (100) is that they are being used as role-type descriptions that do not yet denote anything at all; we are *deciding* which things are going to fill the role and are invoking various criteria.

Contrast (96), 'the book is on the table', with (101), which is more like Bach's two examples.

(101) The book should be on the table.

Is 'the book' in (101) a rigid incomplete definite description or not? I believe that it is not rigid if we're in a situation where we're using 'the book' as a role-type definite description, which would be analogous to the intended reading of Bach's sentence (100). The situation could be one like this. We are at the bookstore buying a gift for a friend. We know that we're going to buy a book, but just *one.* Suppose there's a table that has the sale books on it. Given that you and I have agreed in advance that we want to get a nice book but not spend a lot of money on it, one of us suggests, by uttering (101), that being on the sale table be a necessary condition for being the book we end up deciding to buy.

But contrast this with a situation in which the definite description 'the book' is not being used as a role-type description. In such a situation we would be using the definite description 'the book' rigidly, to refer to our copy of 'Herzog', say, and saying of *it* that it is likely to be on the table (or expected to be and so forth).

Names exhibit a similar phenomenon.

(102) Una should have been your first daughter.[48]

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[48] That is based on an example due to Irene Heim, personally communicated to Paul Elbourne. See Elbourne (2005, 182).

> If John had had five sons, Quintus would have been bullied by the others. (In John's community, there is a convention of naming your fifth son Quintus.)

If we imagine this as said to parents who are considering whether to give their second daughter the name 'Una', then we are using the name as a "role-type" description, analogous to the description 'the party' in the intended reading of Bach's sentence (100) and the description 'the book' in the first described reading of (101).

Covarying Incomplete Definite Descriptions: We sometimes have incomplete definite descriptions that are not used to designate any individual at all, rigidly or otherwise, but rather have a denotation that varies in coordination with an antecedent.

(103) If Carlos meets a scientist and decides to marry her, then his current wife will stalk the scientist and try to kill her.

The definite description 'the scientist' here is acting something like a variable that's "bound" by the antecedent indefinite description 'a scientist'. (I put "bound" in quotes, since binding may well not be the right way to account for this sort of covariance.)

But we also see such covarying uses of names. Recall these from note 22:

(104) If a child is christened 'Bambi', and Disney Inc. hear about it, then they will sue Bambi's parents. (Geurts 1997, 321)

(105) If John insists on calling his next son Gerontius, then his wife will be annoyed and Gerontius will get made fun of because of his name. (Elbourne 2005, 182)

The names in the second clauses of these sentences do not designate any one particular thing. In fact, they're acting just like the "bound" incomplete definite description in (103) above. So here, the nonrigid occurrences of the names in (104) and (105) display just the sort of nonrigid covariance that we see with the incomplete definite description in (103). Again, piggybacking allows us to defer accounting for covarying occurrences of names. Covarying occurrences of names are just a special case of the more general phenomenon of covarying occurrences of incomplete definite descriptions.

The conclusion of these three brief discussions about nonrigid incomplete definite descriptions is that the rigidity thesis requires qualification: names are rigid in just those environments in which incomplete definite descriptions would be.

Names Are Incomplete

Let's review the situation we're now in. We worried that the rigidity of names might falsify 'The'-Predicativism since things have their names only contingently. Since things only contingently have the names that they in fact have, it looked like an incomplete denuded definite description such as '$\varnothing_{the}$ Aristotle' could not be rigid. Aristotle Onassis might not have been given the name 'Aristotle', and therefore might not have been in the extension of that name. This would be a problem for 'the'-predicativists, since on our view a sentence with a singular, bare occurrence of a name 'N' appearing in argument position is semantically equivalent to the corresponding sentence with a denuded incomplete definite description '$\varnothing_{the}$ N' in the same position. In the face of this problem, we noted that incomplete definite descriptions are in general rigid. It is therefore not ad hoc to posit that incomplete (denuded) descriptions like '$\varnothing_{the}$ Aristotle' are rigid.

What we did, therefore, was show that a certain property of names, namely, their rigidity, is one that we would expect names to have if 'The'-Predicativism were true. This argument took this form: names are known to have a certain property; incomplete definite descriptions have that property too; therefore, the fact that names have that property is no problem for 'The'-Predicativism.

Now we'll see an argument that works in the opposition direction, one that has this form: incomplete definite descriptions are known to have a certain property; names have that property too; therefore, the fact that incomplete definite descriptions have that property is no problem for 'The'-Predicativism.

The property in question is *incompleteness*. As normally understood, *incompleteness* is something that by definition applies only to definite descriptions: a definite description 'the F' is incomplete just in case its embedded predicate 'F' is true of more than one thing. Of course if every occurrence of a name 'N' appearing in argument position is semantically equivalent to a definite description '$\varnothing_{the}$ N', then since most names that are used to talk about a particular individual are borne by more than one individual, the definite description '$\varnothing_{the}$ N' will be incomplete. Given such a circumscribed understanding of what incompleteness amounts to, it could make sense to say that a name is "incomplete" only by someone who thought that 'The'-Predicativism was true. So if the incompleteness of names in argument position is to form part of the evidence for 'The'-Predicativism, some more inclusive understanding of incompleteness—

one that applies to names in argument position—is required. So let us say this:

***NP*-Incompleteness:** A noun phrase '*NP*' appearing in argument position is incomplete when, although it is used to talk about a particular individual, it wouldn't be inappropriate to ask of the speaker, "which *NP* are you talking about?" in order to know what was said. Either that, or one could legitimately have taken the speaker to have been talking about something other than what he or she was in fact talking about.[49]

It is not difficult to see that in that sense, names may be incomplete just as definite descriptions may be.

(106) A: Did you bring the book back to the library?
B: Which book are you talking about?
A: The Lemony Snicket book you were reading.

(107) A: Aristotle engaged in unethical business practices as a young man.
B: Which Aristotle are you talking about?
A: Aristotle Onassis, of course.

(108) A: I had a lot of fun at the party.
B: Really?! I thought that Noah's party would be a total bore.
A: No, I'm talking about Naomi's party.

(109) Rewind thirty years.
A: Saul is writing a new book.
B: What?! Saul Kripke is writing a new book?
A: No, I'm talking about the Saul who wrote *Herzog*.

So what's going here? If names appearing in argument positions are rigid designators, how could the names 'Aristotle' or 'Saul', when they occur in argument positions in these sentences, designate more than one individual? If names in these sentences designate the same individual in every possible world, how could they designate more than one individual in the actual world? Obviously, the names 'Aristotle' and 'Saul' designate more than one individual in the actual world in the sense that there is more than one person in the actual world who is called Aristotle and more than one person in the actual world who is called Saul. Kripke's claim that the names 'Aristotle' and 'Saul' are rigid designators must be understood so as not to be falsified by this fact. Kripke's claim is about bare singular unmodified names appearing in argument position. To say that these names are rigid designators, then, must be to say that once their denotation is determined, in a context, to be some particular individual, then the name, as used in that context, designates that same individual in each possible world. This can be put in terms of truth conditions for modal attributions containing that name, as used in a context. For example, if a speaker in a context uses the name 'Aristotle' to designate an individual *o* (Onassis), when uttering 'Aristotle might have been a Lothario', then the name 'Aristotle', as used in that context, will be "rigid" in the sense that the utterance will be true just in case there's a possible world in which *o* is a Lothario. And it can also be put in terms of the modal profile of the "simple" sentence 'Aristotle is a Lothario'. (For discussion of that latter point, see the preface to the 1980 edition of *Naming and Necessity.*)

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[49] This second disjunct of the definition serves to accommodate David Kaplan's (1990) idea that no two people can have the very same name, even when those names sound just alike and are spelled in just the same way. Aristotle Onassis and Aristotle the philosopher have different "common currency names" (a phrase coined by Kaplan), but they are different instances of the same "generic name."

So names are rigid-in-a-context. Recall that we were led to say exactly that about incomplete definite descriptions. Despite this, they are *NP*-incomplete. Likewise, incomplete definite descriptions and, a fortiori, incomplete definite descriptions that embed unmodified singular names, are rigid-in-a-context as well as being *NP*-incomplete.

Our conclusions for this section are, then, that (i) incomplete definite descriptions that embed unmodified singular names are rigid-in-a-context, so the fact that names in argument position are rigid-in-a-context is consistent with their being semantically equivalent to incomplete definite descriptions that embed unmodified singular names, and (ii) names are *NP*-incomplete, so the fact that definite descriptions that embed unmodified singular names are *NP*-incomplete is consistent with their being semantically equivalent to names in argument position.

14. Some Methodological Remarks

There is more than one kind of count noun; there are names and there are common count nouns. I use the terms "name" and "common noun" mutually exclusively because they have different syntactic properties. Their distribution with the *overt* and *covert* definite articles distinguishes them. This is on a par with saying that there is more than one kind of common noun; there are common count nouns and there are common mass nouns. *Their* pluralizability and distribution with determiners distinguishes *them.* So, for example, these sentences are grammatical: 'Cats are the bane of my existence' and 'A cat is on the roof.' But these are not: 'Furnitures are the bane of my existence' and 'A furniture is on the roof.'

A key thesis of 'The'-Predicativism is the *Denuded Definite Description Thesis.* The denuded definite description thesis allows for names to have the semantic value of a predicate in all of their occurrences and to be grammatically categorized as a kind of count noun. The denuded definite description thesis also enables formulation of a simple rule that accounts for the surprising distribution pattern of names with '$\emptyset_{the}$'. The pattern is that the definite article, unless it is stressed, must be realized as '$\emptyset_{the}$' with a singular name unless the name is either preceded by a restrictive or nonrestrictive modifier or followed by a restrictive modifier.

Denuded Definite Description Thesis: Names constitute the nominal component of denuded definite descriptions when they appear as bare singulars in argument position.

Moreover, the denuded definite description thesis affords a neat and simple syntactic generalization to cover the puzzling distribution pattern, namely, that the definite article must be realized as $\emptyset_{the}$ when it has a name as its sister, unless it is stressed.

Where $\emptyset_{the}$: The definite article must be realized as $\emptyset_{the}$ when it has a name as its sister, unless it is stressed.

This gives perhaps the best support for 'The'-Predicativism (as against its 'that' cousin as well as Referentialism). It yields a simple explanation of the distribution of names with the overt definite article 'the'. Predicativism also has the advantage of *uniformity*: it assigns the same predicate-type semantic value to seemingly bare singular names that it assigns to names that are clearly in a predicate position.

Some authors, both for and against, suggest that a "uniformity argument" is one of the best arguments that has been given (or perhaps can be given) for Predicativism.[50] In light of that, it is worth explicitly saying something that probably everyone realizes anyway, namely, that uniformity is not advantageous per se. Uniformity is advantageous just to the extent that it provides for theoretical simplicity along with empirical adequacy.

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[50] See, for example, Burge (1973, 430), Elugardo (2002, sec. 2), Robin Jeshion (forthcoming a, forthcoming b), Kroll (n.d.), Leckie (2013, secs. 1–2), Rami (2014), and Sawyer (2010, 207).

We have two competing theories of names. One of them is nonuniform in maintaining that when names occur as bare singulars in argument position, they are referring expressions but when they occur with determiners, or in the plural, they are predicates. The other theory is uniform in the sense that it assigns a predicative value to a word wherever it occurs as a name. These facts alone do not provide support for one theory over the other. The nonuniform theory of names might well provide for an overall simpler semantic theory than its uniform rival. For example, if there were other types of expressions that seemed to have a semantic variation that's similar to the one that names seem to have, then the simplest theory might well be one that allows for variation in both types of expression while explaining that variation with a single, productive principle. Gail Leckie (2013) has argued that a polysemous theory of names is in fact simpler than a uniform theory for just this reason. She has argued that a more general account of other semantic-shift phenomena would yield an explanation of the semantic shift that's posited by the nonuniform polysemous view. In particular, she invokes the polysemy that we see with botanical names, for example, with 'walnut' being used for a nut, for the kind of tree that produces that nut, or even for a particular individual tree of that kind. Leckie argues that a theory that accounts for that phenomenon could simultaneously account for the polysemy that she proposes for names. That is exactly the right sort of counterargument to the predicativists' claim that uniformity is a good feature of their theory of names.

Robin Jeshion (forthcoming a), in contrast, has argued that predicativists cherry-pick those occurrences of names that justify their own claim for uniformity by discounting those predicative uses of names that do not conform to the being-called condition. For example, at our Halloween party, we say that many of our guests are Obamas but fewer of them are Abe Lincolns, even though those Obamas and Lincolns do not have 'Obama' or 'Abe Lincoln' as a name. So we predicativists cannot claim, the objection goes, to have genuine concerns for uniformity, she argues. We arbitrarily, and therefore unjustifiably, choose to ignore such uses when claiming uniformity for our own view. The point is well taken. But it is not apropos. We predicativists believe that our choice about which occurrences of names are the ones to be unified is the theoretically virtuous choice insofar as—but only insofar as—that choice provides for an overall simpler semantic theory.

Just as 'Obama' can be used as a predicate true of people who have 'Obama' as a name as well as true of people in an Obama costume, the common noun 'witch' can be used as a predicate that's true of witches as well as one that's true of people in a witch costume. (There were three witches and four ghosts at our Halloween party, but only one black cat.) Those cases where a name occurs in predicate position but is not used so as to apply to things that are bearers of that name get just the same treatment as cases where a common noun occurs in predicate position but is not used with *its* standard application conditions.

The aim of uniformity actually plays two roles in my own development and defense of Predicativism. On the one hand, the goal of uniformity plays a causal role in my adopting the theory that I have in fact adopted. On the other hand, the ultimate defense of the theory that I advocate relies on a claim for comparative simplicity that itself results from unifying the particular occurrences of names that I have chosen to unify.

Let me illustrate the distinction with a storied example. In 1865 August Kekulé proposed that the benzene molecule consisted of a ring of six carbon atoms with alternating double and single bonds, each bonded to one hydrogen atom. The structure of the molecule had eluded chemists for many years before Kekulé's discovery. Kekulé claimed that he came to this hypothesis after having a daydream of a snake chasing its own tail. The daydream was what led him to the hypothesis, but was not considered evidence in favor of the hypothesis.

A theorist in any discipline might be led to develop a theory for any number of reasons. Those reasons need not form any part of an ultimate defense of the theory. One scientist might develop a particular theory because he heard voices in his head telling him to do so. His having heard those voices would not later be invoked in defense of the theory (unless perhaps the voices in his head had tended to be correct in the past). Another scientist might develop a particular theory based on a hunch. He trusted his hunches because they'd worked for him in the past. The fact that he had those hunches would not ultimately be involved in defense of the theory.

Yet a third scientist might develop a particular theory because it unified certain phenomena and he had a hunch that the unification of those particular phenomena would ultimately provide for a theory that was simpler overall than its competitors. The hunch is not part of the evidence for the theory.

What I need to defend is the theory that I have developed. There is no onus on me to defend my reasons for developing it, or to defend the theory by appealing to the reasons that led me to develop it—such as an aesthetic preference for unification, accompanied by hunches about which occurrences of names should not fall under the umbrella of such a unification.

15. Conclusion

Let me review what has been accomplished here in defense of Predicativism. First, explication of the theory revealed that it provides a uniform account of names: it assigns the same semantic type (that of a predicate) and applicability condition (the being-called condition) to names when they occur in predicate position as it does when they occur in argument position as bare unmodified singulars.

Second, I addressed an argument against Predicativism that continues to surface: the argument that it is a point against Predicativism that names do not have the same syntax as other count nouns. I countered with an obvious reply: there is more than one kind of count noun. This sort of argument counts no more against Predicativism than it does against *Massism*, the view that mass terms like ‘cutlery’ are common nouns even though they have a different syntax from other common nouns, like ‘dog’.

Third, I presented Sloat’s disjunctive generalization of the syntactic difference between names and common count nouns. I explained why Sloat posited that apparently bare unmodified singular names really complement a null definite article. I argued for two amendments to Sloat’s disjunctive generalization and showed that with those amendments in place, we could give a streamlined, nondisjunctive generalization (“Where $\emptyset_{the}$”) of when the definite article would appear overtly as ‘the’ with a name and when it would be phonologically null. There were two important corollaries here: (A) neither Referentialism nor ‘That’-Predicativism can accommodate the streamlined generalization, and (B) the fact that ‘The’-Predicativism alone can explain the syntactic facts displayed in the Sloat chart means that it has explanatory power that its main rivals do not have.

Fourth, I demonstrated that names and incomplete definite descriptions are rigid in just the same sentential environments as each other, so the rigidity of names is not only compatible with the **Denuded Definite Description Thesis** but predicted by it.

The ultimate defense of ‘The’-Predicativism would consist of more than what I have done here. It would require evaluations of relative simplicity with other theories of names. For example, I have not engaged in any serious way with Leckie's Referentialism, which she calls a "polysemy" view of names. And there are rival views that I have not considered at all. In particular, I have not said anything about the view that names are variables, a view associated with Takashi Yagisawa (1984), Joshua Dever (1998), and Samuel Cumming (2008).

Gabriel Segal has suggested that various psychological data regarding linguistic deficits and development show that names and common nouns are distinguished in cognition. On the present theory, however, names are differentiated from common nouns in at least two relevant respects. First, they are of a different *grammatical* category: names differ syntactically from common nouns since they distribute differently with respect to the overt definite article. Furthermore, names have a distinctive condition of application, as reflected in the being-called condition. Whether or not a name applies to a thing depends on what names the thing has, which in turn depends on certain events having taken place: that the thing has been given a certain label, as it were, in an appropriate way (for example, by its parents, by itself, or by its owner), or that a certain label become regularly used to address or refer to it (for example, by scholars, scientists, or other relevant experts). For those reasons, there being cognitive or neurological differences between names and common nouns presents no special difficulty for 'The'-Predicativism, any more than do cognitive differences between predicates for biological kinds and those for artifact kinds or between predicates that are adjectives and ones that are nouns or between predicates for concrete objects and ones for abstract objects.

An evaluation of the relative simplicity of Referentialism and Predicativism would require a comparison of the complexity and number of different principles required in order for each one to be empirically adequate. If such principles are already needed to support theories of expressions other than names, then they do not increase the complexity of the theory. I have in mind here, for example, principles governing metonymy of various sorts—as when we can use the name 'Obama' as a predicate true of things wearing an Obama costume at a Halloween party just as we can use the common noun 'witch' as a predicate true of things wearing a witch costume at a Halloween party. Further if the theory itself has explanatory power in the sense that other phenomena are explained by it, then that roughly decreases the complexity of the theory. I have in mind here the distribution of names with the overt definite article.

I also have in mind the validity of certain entailment patterns holding between the occurrences of names as bare singulars and the rest. As Jennifer Hornsby (1976, 229) pointed out, (110) entails (111).[51]

(110) Alfred is sane.
(111) Not all Alfreds are crazy.

These "Hornsby entailments" are an immediate consequence of predicativism.[52] We might also note, that in the preceding sentence, the name 'Hornsby' occurs as an attributive noun, just as common count nouns can, as in 'automobile regulations', 'student concerns', and 'school closings'.

And 'The'-Predicativism in particular, as contrasted with 'That'-Predicativism, affords a streamlined syntactic generalization of the Sloat distribution patterns. Furthermore, the rigidity of bare singular names in argument position requires no special, ad hoc principles to support it. We might also add that various metonymic uses of names are explained by already posited principles of (for example) deferred interpretation—as in the costume examples.

Although uniformity is not virtuous per se, we do not yet have a theory of the semantics of names that is simpler or more explanatorily powerful than 'The'-Predicativism.

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[51] The same point has been made, apparently independently, using different examples, by Elugardo (2002, 476), Kroll (n.d.), Leckie (2013), and Pietroski (2007, 353).

[52] I should say, though, that some authors have argued, quite plausibly, that such entailments can already be explained as instances of a more general phenomenon of semantic shift (Leckie [2013], Rami [2014]).

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Emergent Chance

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There has been much debate on whether there can be objective chance in a deterministic world. The "orthodox view" is that nondegenerate objective chance ("true randomness") is incompatible with determinism and that any use of probability in a deterministic world is purely epistemic, reflecting nothing but an observer's lack of complete information. This view was held by Popper (1982) and Lewis (1986) and has recently been defended by Schaffer (2007). Other authors defend "compatibilist views," according to which there *can* be nondegenerate objective chance in a deterministic world (for example, Hoefer 2007, Ismael 2009, Sober 2010, Glynn 2010). They employ a variety of argumentative strategies, ranging from an appeal to statistical mechanics (for example, von Plato 1982, Loewer 2001, Frigg and Hoefer 2010) to a semantic approach linking chance to ability (Eagle 2011).

One strategy is to argue that the objective chance of an event depends on the level of description (for example, Loewer 2001, Glynn 2010, Strevens 2011). According to this strategy, saying that, macroscopically described, a coin toss has an objective chance of ½ of landing heads is consistent with saying that, microscopically described, the initial

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state of the coin determines the outcome. Furthermore, as Glynn (2010) argues, such level-specific chances can play the role we expect "objective chance" to play. However, no existing version of this strategy has been sufficiently immunized against the objection that so-called higher-level chances are best understood, not as true objective chances, but as expressing the observer's uncertain degrees of belief about the events in question, given his or her informational limitations.[1]

We develop an account of objective chance as an emergent phenomenon that answers this objection. Our account is based on a formal model of the relationship between different levels of description of a system (drawing on List 2014 and Butterfield 2012) and shows how indeterminism and chance at a higher level can coexist with determinism and the absence of chance at a lower level.[2] We identify a precise sense in which higher-level chance does not collapse into epistemic probability and show that the distinction between the two can be drawn and operationalized at *every* level of description. It is therefore misleading to draw a single overall distinction between objective chance and epistemic probability. There is an entire *family* of such distinctions: one for each level.

The key insight underlying our account is that different levels of description of a system correspond to different specifications of the system's state space and its set of possible histories, at different levels of "coarse graining," which induce different "algebras of events" on which probabilities are defined. Far from overcomplicating matters, this insight allows us to develop a parsimonious criterion of what separates objective chance from epistemic probability. What we are suggesting is no doubt implicit in earlier work on the topic (for example, von Plato 1982), but the literature does not yet contain a satisfactory account of why the objective-epistemic distinction can be drawn at every level and how different levels are insulated from one another so as to permit objective chance as a higher-level phenomenon, despite "chancy" higher-level world histories supervening on "nonchancy" lower-level ones.

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[1] Another view, defended by Lyon (2011), is that higher-level probabilities, such as those that we find in classical statistical mechanics or evolutionary theory, are neither objective chances nor credences, but *counterfactual probabilities*, whose primary role is to convey certain counterfactual information in explanations.

[2] Butterfield (2012) and List (2014) discuss emergent indeterminism in different contexts, not the context of chance. As we were revising this essay, a new defense of level-specific chance by Frigg and Hoefer (2013) came to our attention, which is somewhat similar in spirit to ours, though without an explicit model of branching histories or level-specific algebras of events. Frigg and Hoefer (2013, 8) defend a "chance-rule pluralism" according to which "probability rules can be formulated in terms pertaining to different levels of discourse such as macro physics, chemistry, genetics, mechanical engineering and meteorology, and probability rules formulated in such terms have equal right to be considered for inclusion in a Best System package of rules, alongside micro-level rules."

1. The Basic Setup

We model a system whose state evolves over time.[3] *Time* is represented by a set T of points that are linearly ordered. The *state* of the system at each time is given by an element of some set S of possible states, which we call the *state space.* A *history* of the system is a temporal path through the state space, formally a function h from T into S, where, for each time t in T, $h(t)$ is the state of the system at t.

In this model, histories play the role of possible worlds. We write Ω to denote the set of all histories deemed *possible.* This could be either the set of all logically possible functions from T into S or, more plausibly, a proper subset of that universal set, so as to capture the fact that the laws of the system permit some histories while ruling out others. Possibility (in Ω) can then be understood as nomological possibility.[4]

It is helpful to view the states in S as the different possible physical states that the system could be in and the histories in Ω as the different possible physical histories. Later, in section 4, we introduce more "coarse-grained" sets $\mathbb{S}$ and $\mathbb{\Omega}$ of "higher-level" states and "higher-level" histories, but S and Ω as introduced here should be understood as containing only states and histories at a single, "lower" level.

To define determinism and indeterminism, some further terminology is needed. For any history h and any time t, we write h_t to denote the *truncated history* up to time t (defined as the restriction of the function h to all points in time up to t in the relevant linear order). A history h is *deterministic* (in Ω) if, for every time t, its truncation h_t has only one possible continuation in Ω, where a *possible continuation* of h_t is a history h' such that $h'_t = h_t$. A history h is *indeterministic* (in Ω) if, for some time t, its truncation h_t has more than one possible continuation in Ω. Thus indeterministic histories allow *branching,* while deterministic histories do not. Note that a history's property of being deterministic or indeterministic is defined relative to the set Ω of possible histories and thereby relative to the underlying laws (which induce Ω). These laws can be said to be *deterministic* if Ω contains only deterministic histories (that is, there is never any branching) and *indeterministic* otherwise (that is, there is sometimes branching).

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[3] We use and extend a formalism that has previously been used in a different context—that of agency and free will—by List (2014). Structurally similar branching-history models include Butterfield (2012) and, in agential contexts, Belnap, Perloff, and Xu (2001).

[4] The laws of the system may go beyond specifying modal facts (facts about what is and is not nomologically possible); the set Ω only encodes those modal facts. A family of objective chance functions, when it exists, may encode additional, probabilistic facts.

Probability functions, irrespective of their interpretation, are always defined on algebras of events. An *event* is a collection of histories, that is, a subset of Ω. An *algebra* is a collection of events that is closed under union, intersection, and complementation. One example of an algebra is the set of all possible events (that is, the power set of Ω). However, when Ω is infinite, it is technically useful to work with smaller algebras. Typically, the structure of Ω dictates a canonical choice of algebra, which we label $\mathcal{A}(\Omega)$.[5] A *probability function* is a function Pr from $\mathcal{A}(\Omega)$ into the interval from 0 to 1 with standard properties; $\Pr(E)$ denotes the probability of event E. The function is *nondegenerate* if some events have probability greater than 0 and less than 1.

There can be different probability functions on the same algebra, indexed to different "locations" or "vantage points." It is widely agreed, for example, that any objective chance function, when it exists, is indexed to a particular history and time (Lewis 1986, Schaffer 2007). To indicate this, we use the notation $\Pr_{h,t}$. Chance assignments thus take the form "event E has objective chance p in history h at time t" (in short, $\Pr_{h,t}(E) = p$).[6] Epistemic probability (or credence) functions are indexed to agents and their informational states (and optionally histories and times). Assignments of epistemic probability (or credence) thus take the form "agent A with information I (in history h at time t) has degree of belief p in event E."[7] For the moment, we do not need any explicit notation for epistemic probability functions.

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[5] For example, if Ω has a topology, $\mathcal{A}(\Omega)$ is usually the Borel sigma algebra.

[6] An alternative approach, also consistent with our analysis, is to take conditional chance as basic (for example, Hájek 2003a, 2003b). On this approach, one need not explicitly *define* a chance function $\Pr_{h,t}$ for each history h and time t, but can *derive* it from a family of conditional probability functions $\Pr(\cdot \mid \cdot)$ by conditionalizing on h_t (that is, on the event $\{h' \in \Omega\colon h'$ is a continuation of $h_t\}$); for any event E, $\Pr_{h,t}(E) = \Pr(E \mid h_t)$. If we take $\Pr(\cdot \mid \cdot)$ as basic, the functions $\Pr_{h,t}$ inherit Bayesian restrictions (for example, if t is after t', $\Pr_{h,t}(E) = \Pr_{h,t'}(E \mid h_t)$), a slight (but perhaps plausible) loss of generality. For analyses of chance via conditionalization on histories, see Loewer (2001, esp. 618), Hoefer (2007, esp. 562–65), and Glynn (2010, esp. 78–79).

2. Objective Chance

When does a history-and-time-indexed probability function $\Pr_{h,t}$ qualify as an objective chance function? Consider a family of such functions, $\langle \Pr_{h,t} \rangle$, with h ranging over the histories in Ω and t ranging over the times in T. Schaffer (2007) proposes six desiderata that this family must satisfy to play the "objective chance" role.[8] These express the idea that chance must relate in the right way to various other pertinent concepts, such as credence, possibility, the future, intrinsicness, lawfulness, and causation. For present purposes, we accept Schaffer's claim that whichever family of probability functions "best" satisfies these desiderata represents objective chance. We call such a family an *(objective) chance structure* on Ω. A lot more could be said about the desiderata than space constraints allow us to say here, but since they are the starting point of Schaffer's critique of deterministic chance, they also serve as a useful starting point for us. Adapted to our framework,[9] the desiderata are as follows:

> **The chance-credence desideratum:** If an agent, in history h at time t, were to receive the information that the objective chance of some event $E \subseteq \Omega$ is p, he or she would assign a degree of belief of p to E, no matter what other admissible information he or she has (where this information is

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[7] If one takes agent A's information I to "screen off" the history h and time t where A is located, one can drop the indices h and t. This would slightly reduce generality by ruling out the possibility that, in different histories or at different times, the same information I may lead to different credences, for example, due to different priors or psychological states of A.

[8] Schaffer speaks of a single function with three arguments: a proposition (event), a world (history), and a time. Technically, only the projection of this function for a fixed history and a fixed time is a probability function; so it is more correct, though equivalent to Schaffer's usage, to speak of a *family* of history-and-time-indexed probability functions.

[9] Schaffer speaks of each world (history, in our terms) having laws. In our model, laws enter in two ways: (i) they impose modal constraints on what histories are nomologically possible, determining Ω; (ii) they determine the chance structure. As in Schaffer's analysis, some properties can be attributed to histories only relative to the laws. For instance, whether a history is branching depends on the laws encoded in Ω. Similarly, the chance of an event E in history h at time t depends on the laws encoded in the chance structure.

> formally represented by a subset $I \subseteq \Omega$, containing precisely the histories consistent with the information).[10]

This is a version of Lewis's "Principal Principle," which is commonly accepted as a key constraint on the role of chance. The second desideratum is equally natural: only events that are possible can have nonzero chance.

> **The chance-possibility desideratum:** A necessary condition for an event $E \subseteq \Omega$ to have nonzero objective chance in history h at time t is that E is *possible* in h at t, meaning that E contains a continuation of h_t.

This implies further that only contingent events can have nondegenerate chance in history h at time t, where an event E is *contingent in history h at time t* if E and its complement are each possible in h at t. (Note that this is a notion of contingency *relative to a history and time.*) The third, and related, desideratum says that only future events can have nondegenerate chance.

> **The chance-future desideratum:** A necessary condition for an event $E \subseteq \Omega$ to have nondegenerate objective chance in history h at time t is that E is "properly in the future" in h at t.

Spelling out what it means for an event E to be "properly in the future" in history h at time t is a nontrivial task, but for a variety of criteria the chance-future desideratum is a consequence of the chance-possibility desideratum. This is easy to see for a simple but perhaps unsatisfactory criterion, according to which an event is in the future in history h at time t just in case it is contingent in h at t. However, the implication also holds for more sophisticated criteria, provided the set of contingent events in history h at time t is a *subset* of the set of future events in h at t.[11] The chance-future desideratum could be problematic in a relativistic spacetime, where the distinction between "past" and "future" depends on the reference frame of the observer, but we set this complication aside.

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[10] We adopt a permissive definition of "admissible information," deeming any information about the past admissible: $I \subseteq \Omega$ is *admissible* in history h at time t if I contains at least all possible continuations of h_t. A more restrictive definition would only strengthen our (positive) conclusions, by weakening the chance-credence desideratum. Since the laws of the system are encoded in Ω (and the chance structure $\langle \Pr_{h,t} \rangle$), the admissible information $I \subseteq \Omega$ can also convey information about the laws. An agent in a deterministic history can thus, in principle, fully predict the future *if* he or she learns which truncated history he or she is in.

[11] One criterion of the future is the following. Let S^T be the set of *all* functions from T into S, that is, the set of all histories that are *logically* possible, given the state space S and the set of time points T; S^T is a superset of Ω. Any event $E \subseteq \Omega$ is (nonuniquely) representable as $E = E^* \cap \Omega$ for some $E^* \subseteq S^T$. Heuristically, E^* is a (merely) *logically* possible event, while E is the set of all *nomologically* possible histories in E^*. For any time t, let $B(t)$ be the set of all times up to and including t, and $A(t)$ the set of all times after t. Let $S^{B(t)}$ be the

The fourth desideratum captures the idea that the objective chance of any event is determined by relevant properties of the event itself, not by extrinsic or relational properties.

> **The chance-intrinsicness desideratum:** For any histories h, h', any events E, $E' \subseteq \Omega$, and any times t, t', if the triple (E, h, t) is an intrinsic duplicate of the triple (E', h', t'), the objective chance of E in h at t is the same as that of E' in h' at t'.

The precise definition of an "intrinsic duplicate" is difficult and raises a number of philosophical issues beyond the scope of this essay.[12] Informally, if all intrinsic properties of (E, h, t) are exactly replicated in (E', h', t'), for instance in two separate runs of the same experiment, then the objective chance facts should be the same.

The fifth desideratum requires that objective chances must be determined by the laws of the system, as opposed to, for instance, the attitudes of the observer.

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set of all functions from $B(t)$ into S, and $S^{A(t)}$ the set of all functions from $A(t)$ into S. Relative to S and T, $S^{B(t)}$ is the set of all *logically* possible truncated histories up to and including time t, and $S^{A(t)}$ the set of logically possible future histories after time t. Then $S^T = S^{B(t)} \times S^{A(t)}$. An event E is *settled in the past* of t if $E = (P \times S^{A(t)}) \cap \Omega$ for some $P \subseteq S^{B(t)}$; E is *properly in the future of* t if it is not settled in the past of t. (If all histories in Ω are *deterministic*, any event $E \subseteq \Omega$ is settled in the past in this sense.)

[12] Schaffer restricts the desideratum to event-world-time triples in which the world is the same. In his model (where worlds are endowed with laws), the same laws then apply to those triples. Since in our model the laws are encoded in Ω and the chance structure, the same "global" laws apply to any triples. Our formulation of the chance-intrinsicness desideratum can be made more or less demanding, depending on our criterion of when (E', h', t') and (E, h, t) are intrinsic duplicates. One operationalization involves (i) specifying a set Π of permutations (one-to-one, onto functions π) on the state space S such that each π in Π induces a permutation π^* on S^T satisfying $\pi^*(\Omega) = \Omega$, and (ii) interpreting the elements of Π as transformations of the system's state that preserve all causally relevant features. Different specifications of Π encode different specifications of what those features are. Examples of π in Π might be shifting all particles in the universe five meters in a specific direction or assigning a unique integer number to every electron and exchanging the even- and odd-numbered electrons. Given a specification of Π, we can define (E', h', t') to be an *intrinsic duplicate* of (E, h, t) if there is some permutation π in Π such that $h'(t') = \pi(h(t))$ and $E' = \pi^*(E)$. The chance-intrinsicness desideratum then becomes the requirement that the permutations in Π be symmetries of the system, where π is a *symmetry* if, for any h in Ω, t in T, and $E \subseteq \Omega$, $\Pr_{h',t}(\pi^*(E)) = \Pr_{h,t}(E)$ with $h' = \pi^*(h)$.

> **The chance-lawfulness desideratum:** There is a set of laws at the level of Ω that determines the chance structure on Ω.

For example, there are physical laws that imply that a photon has a chance of ½ of passing through each of the two symmetrical slits in the classic double-slit experiment.

The final desideratum, as stated by Schaffer (2007, 126), requires that "if a given chance is to explain the transition from cause to effect, that chance must concern some event targeted within the time interval from when the cause occurs, to when the effect occurs." However, formalizing this is difficult, and several authors have criticized the desideratum as either unclear or implausible (for example, Glynn 2010, Frigg and Hoefer 2013). As Glynn (2010, 76) notes, moreover, Schaffer's formulation is not "the obvious candidate for *the* platitude connecting causation and chance," and a better candidate is the following: "causes (tend to) raise the chance of their effects." We therefore replace Schaffer's desideratum with a variant of Glynn's alternative:

> **Chance-causation desideratum:** If, in history h at time t, some event C is positively causally relevant to another event E, then (except in a case of redundant causation) the chance of E, conditional on C, is greater than the unconditional chance of E.[13]

Note that this desideratum only says that C's raising the chance of E is a *necessary* condition for C's positive causal relevance to E (setting aside redundant causation). It does not say that C's raising the chance of E is a *sufficient* condition for positive causal relevance. On most accounts, causation goes beyond a simple probabilistic relationship.[14]

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[13] One might further require that if C is negatively causally relevant to E, then (except in a case of redundancy) the chance of E, given C, is lower than its unconditional chance.

[14] We are greatly indebted to a referee for helping us improve our discussion of the six desiderata. For completeness, here is our adaptation of Schaffer's "causal transition" desideratum: if some event C is probabilistically causally relevant to another event E in history h at time t, then C must happen after time t and before E. (On the time of an event, see n. 11.) This is a consequence of the chance-future desideratum if we adopt the following simple definition: *C is probabilistically causally relevant to E* in history h at time t (before E's occurrence) if C occurs before E and $\mathrm{Pr}_{h,t}(E|C) \neq \mathrm{Pr}_{h,t}(E)$. The "causal transition" desideratum then asserts, in essence, the *Markov property* of a stochastic process: all "background causes" before time t are encoded in the truncated history h_t and no longer count as probabilistically causally relevant *once time t in history h is reached.* Four caveats are due. First, the claim that the "causal transition" desideratum is implied by the chance-future desideratum holds if we disallow $\mathrm{Pr}_{h,t}(C) = 0$ (to avoid complications

If we accept the six desiderata, we obtain the following conclusion, as noted by Schaffer (2007).

> **Observation 1:** There can be no nondegenerate objective chance in a deterministic history.

To see this, let h be a deterministic history, and consider, for example, the chance-credence desideratum. If an agent were to receive the information that some event E has nondegenerate objective chance p in history h at time t, he or she would have to assign a degree of belief of p to E, no matter what other admissible information he or she has. However, the full information about the truncated history up to time t is certainly admissible.[15] Formally, this is the subset I of Ω consisting of all possible continuations of h_t. But h is deterministic, so I is the singleton set containing only h itself. Thus, conditional on I, the agent will assign credence 0 or 1 to E, depending on whether E contains h or not. This contradicts the chance-credence desideratum, which mandated a credence p strictly between 0 and 1.

Similarly, consider the chance-possibility desideratum. We have already noted that it implies that only events that are contingent in history h at time t can have nondegenerate chance in h at t. But if h is deterministic, no event is contingent in h at t. This is because the truncation h_t at any time t has only one continuation, namely, h itself, and thus any event E is possible in h at time t if and only if E contains h itself, in which case the complement of E is impossible. So, in a deterministic history, the chance-possibility desideratum rules out nondegenerate objective chance.

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raised by conditionalizing on possible but zero-probability events). Second, we require C to occur before E to rule out backward causal relevance (though this may be contested; see, for example, Price 2008). Third, the probabilistic causal relation depends on a history and time because certain background conditions may be necessary for C to become probabilistically causally relevant to E; for example, lighting a match cannot "cause" a fire without flammable material nearby. Fourth, *probabilistic* causal relevance must be distinguished from causal relevance *simpliciter*, since the latter may go beyond the former. See also Pearl 2000.

[15] Proponents of higher-level objective chance typically deny this, deeming only higher-level information admissible (for example, Loewer [2001], Hoefer [2007], and Glynn [2010]). In section 5, we offer a subtly different proposal on how higher-level descriptions can be "insulated" from lower-level descriptions. Instead of defining probabilities on a single algebra of events and restricting the criterion of admissible information, we move from the original, fine-grained algebra to a more coarse-grained, higher-level algebra of events. *Relative to each level,* we can then preserve a permissive criterion of admissible information.

Finally, consider the chance-future desideratum. In a deterministic history, all events are "settled in the past" (as technically explicated in note 11), and thus no event counts as being "properly" in the future. This would also follow if we defined an event's being "properly in the future" simply as requiring that it be contingent in the relevant history at the relevant time; as we have seen, in a deterministic history, no event has this property. Either way, the necessary condition for nondegenerate objective chance, according to the chance-future desideratum, cannot be met under determinism. (This is not to deny that, in a more richly described ontology of events, some events could count as being "in the future" in some other sense, even under determinism.)

The question of whether, in a deterministic history, the other three desiderata—chance-intrinsicness, chance-lawfulness, and chance-causation—can be met by a nondegenerate chance structure is less straightforward. But in any case, it is clear that our package of six desiderata cannot be satisfied in its entirety. By contrast, in indeterministic histories, the desiderata pose no such restriction.

> **Observation 2:** There can be nondegenerate objective chance in an indeterministic history.

To see this, it suffices to consider an example. Take a toy universe containing only one particle, whose state is fully described by its location. Space and time in that universe are both discrete, and space is one-dimensional. Thus, spatial positions can be represented by integers and points in time by positive integers. In other words, we can write $S = \{\ldots, -3, -2, -1, 0, 1, 2, 3, \ldots\}$ and $T = \{1, 2, 3, \ldots\}$. For the purposes of our example, we suppose that the set Ω of nomologically possible histories consists of all histories where the particle begins at some spatial position s in S at time $t = 1$, and then moves exactly one spatial position (either left or right) in each time period. These histories are nondeterministic, because any truncated history of length t can be extended in Ω to a truncated history of length $t + 1$ in two ways. For example, the truncated history (0, 1, 2) can be extended both to (0, 1, 2, 3) and to (0, 1, 2, 1). We complete the example by supposing that the laws of this toy universe, over and above inducing the set Ω, determine the following chance structure.

For any time t and any spatial position s, let $E_{[s \text{ at } t]}$ be the set of all histories h in Ω such that $h(t) = s$. Now let h be a specific history in Ω, and suppose $h(t) = s$. We suppose that the laws then specify $\Pr_{h,t}(E_{[s+1 \text{ at } t+1]}) = ½$ and $\Pr_{h,t}(E_{[s-1 \text{ at } t+1]}) = ½$. In other words, the particle has an equal chance of moving right or left in each period.

The chance function $\Pr_{h,t}$ is given by multiplying these "one-step" chances in the obvious way. To be precise, for any positive number n, there are 2^n possible extensions of any truncated history h_t of length t to a truncated history of length $t+n$, and the chance function $\Pr_{h,t}$ will assign probability $1/2^n$ to each of these possible extensions. There is no barrier for the resulting family of chance functions to satisfy all of the six desiderata listed above. This should, of course, be uncontroversial.

3. Epistemic Probability

We have seen that nondegenerate objective chance can exist only in indeterministic histories in Ω. This does not mean, however, that nondegenerate probability assignments are never appropriate in deterministic histories: they may reflect our uncertain degrees of belief, given incomplete information. In fact, whether or not a history is deterministic, an agent's *epistemic* probability (or credence) function will typically be nondegenerate, unless the agent has complete information *and* there are no chance events in the world.

For example, after having watched the first half of an old recorded football match, we may assign probability ⅔ to our favored team's winning—a nondegenerate probability assignment—despite understanding that the outcome of the match is long settled: we just do not know what it is. This probability assignment simply expresses our uncertain degrees of belief; we are even dealing with a past event. On the other hand, we may also hold nondegenerate epistemic probabilities in cases involving real chance. Consider our disposition to bet on the outcome of tomorrow's football match. Ordinarily, we think that, over and above the players' skills, there is some real randomness involved here. Generally, therefore, epistemic probabilities (or credences) reflect a mix of incomplete information and objective-chance hypotheses.

Clearly, epistemic probabilities need not, and typically will not, satisfy the six desiderata on objective chance. Instead, they must satisfy the following desideratum, which ensures compatibility between an agent's epistemic probabilities (or credences) and his or her information:

> **Epistemic probability-possibility desideratum:** A necessary condition for an event $E \subseteq \Omega$ to be assigned nonzero epistemic probability by agent A with information I (at time t in history h) is that E is *epistemically possible* from A's perspective, meaning that $E \cap I$ is nonempty.

Furthermore, an agent's epistemic probabilities may be constrained by the chance-credence desideratum if they are to respect the agent's knowledge of chance facts. One may or may not wish to impose other desiderata on epistemic probabilities, such as Bayesian ones, but we need not take a stand on this here. The key point is that even in the limiting case of a completely deterministic history, nondegenerate epistemic probability is still possible—and typically entirely rational, given an agent's incomplete information. As long as the information set I is nonsingleton, there is no conflict between nondegeneracy and the epistemic probability-possibility desideratum. The earlier example of the prerecorded football match illustrates this: none of the conceivable outcomes of the match is ruled out by our information after watching the first half.

The example also motivates a simple criterion for distinguishing "pure" epistemic probability from probability assignments that are driven, at least in part, by objective-chance hypotheses. If we had complete information about the history of football, we would already know the outcome of the prerecorded match before we even watched it: there would be no room for nondegenerate epistemic probabilities. However, *even* with complete information about football history, we would *still* not know the outcome of tomorrow's match. We would continue to entertain nondegenerate epistemic probabilities here, which arise from objective-chance considerations.[16] (For those who prefer a microphysical example: we certainly do not know which of the two slits in the classic double-slit experiment a photon will pass through. But even with complete information about the past, we would still assign nondegenerate probabilities to the two possibilities, since—at least on standard interpretations—we are dealing with objective chance.)

In general, a sufficient condition for a nondegenerate probability assignment made in history h at time t to some event E to be *purely epistemic* is that it becomes degenerate, once we conditionalize on complete information about the truncated history up to time t.[17] Formally, this yields the following test:

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[16] There is no guarantee, of course, that such full-information epistemic probabilities will match the "true" objective chances. The fact *that* we assign nondegenerate epistemic probabilities to the different outcomes here is driven by objective chance; the question of *which* probability values we assign reflects our beliefs about the laws of the system.

[17] Thus an assignment of nondegenerate probability counts as "purely" epistemic if it stems from incomplete information alone, rather than any chance considerations. In an earlier analysis of epistemic probability, Skyrms (2000, 26) defines the "*epistemic* prob-

Test for pure epistemic probability: Let Pr be a probability function held by an agent A in history h at time t, and suppose $\Pr(E)$ is nondegenerate for some event E. A *sufficient condition* for Pr to be *purely epistemic* with respect to E is that $\Pr(E|h_t) = 0$ or 1. Here, $\Pr(E|h_t)$ is shorthand for $\Pr(E|I)$, where I is the information that the truncated history is h_t; formally, $I = \{h' \in \Omega\colon h'$ is a continuation of $h_t\}$.

4. Emergent indeterminism

So far, we have described the system of interest at only one level, interpretable as the *lower level.* The state space S can be understood as the set of all possible microphysical states and Ω as the set of all possible microphysical histories. Often, however, we wish to employ *descriptions at some higher level,* for example, by describing the state of water as liquid or frozen, rather than as a complex configuration of individual molecules, or by describing a tossed coin as landing heads or tails, rather than as following a particular finely specified physical trajectory. We now focus on some such higher level of description, for example, the one used in some special science. (Of course, there can be many different levels of description; more on this later.)

We assume that (i) higher-level states and histories *supervene* on lower-level states and histories (meaning that there cannot be any variation at the higher level without variation at the lower level), and (ii) higher-level states are typically *multiply realizable* by lower-level states. There are many different configurations of water molecules that each instantiate the same state of liquid water. Similarly, there are many different physical trajectories of a coin that all correspond to landing heads.

The relationship between the lower and higher levels can be formally captured by the idea of *coarse graining*: each higher-level state corresponds to an equivalence class of lower-level states, consisting of all its possible lower-level realizations. The higher level of description thus induces a partition of the lower-level state space S into some set of equivalence classes. We call such a partition a *coarse graining* of S, and write $\mathbb{S}$ to denote the set of all equivalence classes under it. Each $\mathbb{s}$ in $\mathbb{S}$ represents

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ability of a statement" as the "*inductive* probability of that argument which has the statement . . . as its conclusion and whose premises consist of . . . our stock of knowledge." Our test for "pure" epistemic probability is consistent with this, since, on Skyrms's definition, a nondegenerate epistemic probability that is *solely* due to incomplete information also becomes degenerate if we conditionalize on complete information.

one higher-level state, so that we can interpret the set $\mathbb{S}$ as the higher-level state space (note the outlined letters for higher-level objects).[18] Let σ denote the function that maps each lower-level state s in S to the corresponding higher-level state $\mathbb{s}$ in $\mathbb{S}$ (formally, the equivalence class to which s belongs). The function σ can also be interpreted as a supervenience relation that maps subvenient lower-level states to their supervenient higher-level counterparts.

Next consider histories. Just as a lower-level history is a temporal path through the lower-level state space, formally a function h from T into S, so a higher-level history is a temporal path through the higher-level state space, formally a function $\mathbb{h}$ from T into $\mathbb{S}$. For each time t in T, $\mathbb{h}(t)$ is the higher-level state of the system at t. Note that the supervenience relation σ applies not only to states but also to histories. For each lower-level history h in Ω, the corresponding higher-level history $\mathbb{h}$ is the function from T into $\mathbb{S}$ such that, for each t in T, $\mathbb{h}(t) = \sigma(h(t))$. The set of all possible higher-level histories is the projection of the set Ω of all possible lower-level histories under the function σ, formally $\mathbb{\Omega} = \sigma(\Omega)$. Further, just as the set Ω of lower-level histories can be associated with an algebra $\mathcal{A}(\Omega)$ of lower-level events, so the set $\mathbb{\Omega}$ of higher-level histories can be associated with an algebra $\mathcal{A}(\mathbb{\Omega})$ of higher-level events.[19]

With these definitions in place, all the concepts, definitions, and observations from the previous sections carry over, *without any formal changes*, to the system described at the higher level, where $\mathbb{S}$ is now the state space, $\mathbb{\Omega}$ the set of possible histories, and $\mathcal{A}(\mathbb{\Omega})$ the algebra of events. All the relevant symbols in those sections must simply be replaced by their outlined counterparts.

For example, we can define determinism and indeterminism in higher-level histories in exact analogy to determinism and indeterminism in lower-level histories: a higher-level history $\mathbb{h}$ is *indeterministic* (in $\mathbb{\Omega}$) if, for some time t, its truncation $\mathbb{h}_t$ has more than one possible continuation in $\mathbb{\Omega}$ and *deterministic* (in $\mathbb{\Omega}$) otherwise. A *possible continuation* of $\mathbb{h}_t$ is defined, as before, as a history $\mathbb{h}'$ in $\mathbb{\Omega}$ such that $\mathbb{h}'_t = \mathbb{h}_t$. Similarly, a higher-level event $\mathbb{E} \subseteq \mathbb{\Omega}$ is *possible* in history $\mathbb{h}$ at time t if $\mathbb{E}$ contains a continuation of $\mathbb{h}_t$.

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[18] The representation of higher-level states as equivalence classes of lower-level states (adapted from List 2014) is the simplest mathematical way of capturing the assumption that higher-level states supervene on lower-level states and are multiply realizable by them. We need not take a stand on whether higher-level states *are* equivalence classes of lower-level states or whether they are merely *mathematically represented by* such equivalence classes. Our analysis is compatible with either interpretation.

[19] Formally, we require the function σ to be *measurable* with respect to $\mathcal{A}(\Omega)$, meaning that the inverse image $\sigma^{-1}(\mathbb{h})$ (that is, $\{h$ in Ω: $\sigma(h) = \mathbb{h}\}$) is an element of $\mathcal{A}(\Omega)$ for every $\mathbb{h}$ in $\mathbb{\Omega}$. We can then define $\mathcal{A}(\mathbb{\Omega}) = \{\sigma(E)$: E is any event in $\mathcal{A}(\Omega)\}$.

A key point to note is that determinism at the lower level (in Ω) is fully compatible with indeterminism at the higher level (in $\mathbb{\Omega}$).

> **Observation 3:** For suitable σ (and sufficiently large Ω), there can be indeterministic histories in $\mathbb{\Omega}$ even when all histories in Ω are deterministic (List 2014; for a structurally similar result, see Butterfield 2012).

Since this observation is—in mathematical terms—a possibility result, it can be proved by giving an explicit example of a set Ω of deterministic lower-level histories and a coarse-graining function σ for which the resulting set $\mathbb{\Omega}$ of higher-level histories, where $\mathbb{\Omega} = \sigma(\Omega)$, contains some indeterministic histories. Figure 1 provides such an example. Part (a) shows a simple system at the lower level of description (Ω). Time is plotted on the horizontal axis ($T = \{1, 2, 3, \dots\}$) and the state of the system on the vertical one. Here the state space S is the set of all real numbers. The figure displays five deterministic histories, from time $t = 1$ to time $t = 25$. Part (b) shows the same system at a higher level of description ($\mathbb{\Omega}$), obtained by coarse graining the state space S. Specifically, $\mathbb{S}$ is the set of all integers. The function σ maps each real number s in S to the closest integer $\mathbb{s}$ in $\mathbb{S}$ (with the usual rounding convention). In this coarse-grained description, there are now five indeterministic histories, supervenient on the lower-level deterministic ones. In particular, they all coincide up to time $t = 9$ before diverging from one another.

5. Objective Chance at a Higher Level

Observation 3 shows that while a system may be deterministic at a lower level of description, indeterminism can emerge at a higher level: while the set Ω may contain only deterministic histories, a suitable coarse graining may yield a set $\mathbb{\Omega}$ of indeterministic histories. But then Observation 2, applied to the level of $\mathbb{\Omega}$ rather than Ω, shows that $\mathbb{\Omega}$ may admit a nondegenerate objective chance structure. Thus, nondegenerate objective chance is possible at a higher level of description, *even if the system is totally deterministic at a lower level of description.*

> **Corollary of Observations 2 and 3:** There can be nondegenerate objective chance in a higher-level history (in $\mathbb{\Omega}$), even when all lower-level histories (in Ω) are deterministic. (A necessary condition for this is that the higher-level history is indeterministic, which is compatible with lower-level determinism.)

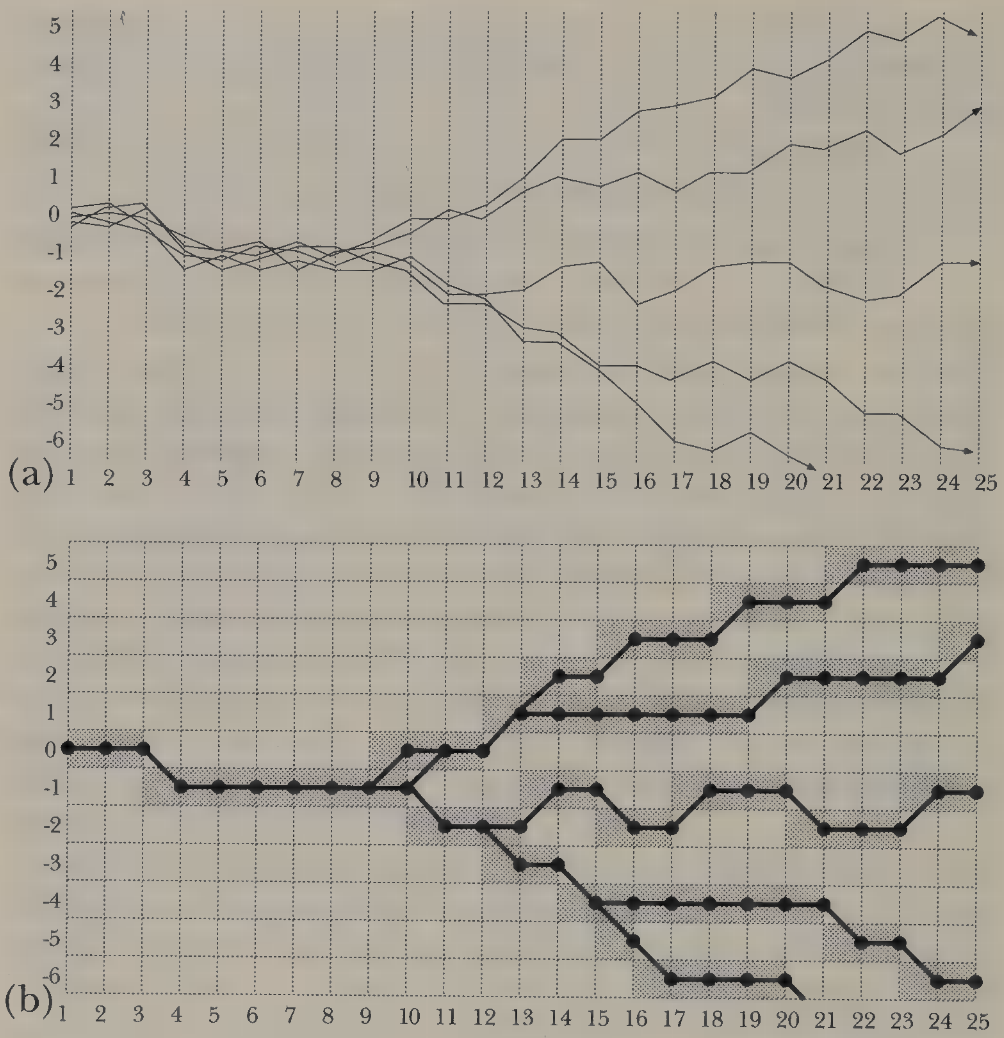

Figure 1. Emergent indeterminism

At first sight, this conclusion may seem puzzling. Have we not established that when the histories in Ω are deterministic, only degenerate objective chance structures can meet the six desiderata? However, the key insight is this: when evaluating chance and (in)determinism at a higher level of description, *only higher-level language is available.* The relevant family of history-and-time-indexed probability functions now consists of functions defined on the algebra $\mathcal{A}(\mathbb{\Omega})$ of higher-level events rather than the lower-level algebra $\mathcal{A}(\Omega)$, and the index $\mathbb{h}$ now ranges over $\mathbb{\Omega}$ rather than Ω (while t continues to range over T). To make this explicit, we write $\langle \mathbb{Pr}_{\mathbb{h},t} \rangle$ to denote the family of higher-level probability functions (on $\mathcal{A}(\mathbb{\Omega})$, with $\mathbb{h}$ ranging over $\mathbb{\Omega}$), as distinct from the family $\langle \mathrm{Pr}_{h,t} \rangle$ of lower-level probability functions (on $\mathcal{A}(\Omega)$, with h ranging over Ω). Our entire analysis from the previous sections, including the desiderata, must then be reapplied at the level of $\mathbb{\Omega}$ rather than Ω.[20]

Past arguments for the *incompatibility* of higher-level objective chance and lower-level determinism tended to make a conceptual error: they supposed that when evaluating the chance of some higher-level event $\mathbb{E} \subseteq \mathbb{\Omega}$, we could employ a lower-level probability function $\mathrm{Pr}_{h,t}$, indexed to a lower-level history h, or conditionalize on a lower-level event, as in expressions of the form "$\mathrm{Pr}_{h,t}(\mathbb{E}) = 0$" or "$\mathrm{Pr}_{h,t}(\mathbb{E} \mid E) = 0$." But it should now be clear that this is misguided. Such expressions involve a category mistake: they mix two different levels of description.[21]

The obstacle here is conceptual, not epistemic. There are, of course, a number of epistemic questions about whether, and why, we should employ higher-level descriptions (lower-level information may or may not be accessible to us, higher-level descriptions may or may not be "reducible" to lower-level ones, and so on). We turn to these issues in section 7. However, the conceptual point is that *when we are operating at the higher level of description, lower-level language is unavailable.* The chances of higher-level events are given by functions of the form $\mathbb{Pr}_{\mathbb{h},t}$, indexed to a higher-level history $\mathbb{h}$ and defined on the higher-level algebra $\mathcal{A}(\mathbb{\Omega})$. So, our claim, at this stage of the argument, is conditional: *if* we are operating at the higher level, we must stick to it.

As further evidence of the pitfalls of mixing levels, note that expressions like "$\mathbb{Pr}_{\mathbb{h},t}(E) = 0$" or "$\mathbb{Pr}_{\mathbb{h},t}(\mathbb{E} \mid E) = 0$" are not even mathematically well defined when the probability function $\mathbb{Pr}_{\mathbb{h},t}$ is indexed to a higher-level history $\mathbb{h}$ and defined on the higher-level algebra of events $\mathcal{A}(\mathbb{\Omega})$, while the event E to which a probability is assigned or on which the probability is conditionalized is described at the lower level. Technically, lower-level events are not in the domain of the probability function $\mathbb{Pr}_{\mathbb{h},t}$.

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[20] Note that this is subtly different from the approach to higher-level chance in, for example, Loewer 2001, Hoefer 2007, and Glynn 2010, which consists in taking a probability function on the original algebra $\mathcal{A}(\Omega)$ and conditionalizing on higher-level information. This still yields probabilities defined on $\mathcal{A}(\Omega)$, though conditional on higher-level information. By contrast, the algebra $\mathcal{A}(\mathbb{\Omega})$ on which we define higher-level probability functions (indexed to histories in $\mathbb{\Omega}$) is isomorphic to a *subalgebra* of $\mathcal{A}(\Omega)$.

[21] Even Glynn's (2010) defense of "indeterministic chance," whose claim about the level-specificity of chance in response to Schaffer (2007) we agree with, preserves the quantification over lower-level histories (worlds) and introduces different levels only via level-specific laws, not via explicit coarse graining of states and histories (worlds) and the move to a higher-level algebra.

In sections 1 and 2, we laid out a theory of objective chance in the setting of indeterministic histories in Ω. But this theory applies equally well to $\mathbb{\Omega}$: simply replace every symbol with its outlined counterpart. As we have seen, when the higher-level analysis (in $\mathbb{\Omega}$) is correctly insulated from lower-level descriptions (in Ω), higher-level indeterminism can coexist with lower-level determinism. So, the possibility of higher-level nondegenerate objective chance follows immediately from the "outlined letter" version of the framework in sections 1 and 2.

We now give a simple example of emergent chance (familiar from the dynamical-systems literature).[22] Consider a system whose state space S is the interval of all real numbers between 0 and 1. Time is given by the set of positive integers, $T = \{1, 2, 3, \dots\}$. The system changes its state from one time period to the next via a *transition rule*, which is formally a function f from S into itself. If s is the state at time t, then $f(s)$ is the state at time $t+1$. Thus, starting at any state s in S, we obtain the following history:

$$h(1) = s,\ h(2) = f(s),\ h(3) = f(f(s)),\ h(4) = f(f(f(s))),\ \text{and so on.}$$

The set Ω is the set of all histories that can be obtained in this way. The system is clearly deterministic.

More specifically, suppose that f is defined as follows (as illustrated in figure 2):

$$f(s) = \begin{cases} 2s \text{ if } 0 \le s \le {}^1/_2; \\ 2 - 2s \text{ if } {}^1/_2 < s \le 1. \end{cases}$$

Now we introduce a coarse graining of this system. Let A and B be symbols representing higher-level states, and let $\mathbb{S} = \{A, B\}$. Define the function σ from S to $\mathbb{S}$ by setting

$$\sigma(s) = \begin{cases} A \text{ if } 0 \le s \le {}^1/_2; \\ B \text{ if } {}^1/_2 < s \le 1. \end{cases}$$

By implication, σ maps each lower-level history h to a higher-level history $\mathbb{h}$ that takes the form of a sequence of As and Bs. For example, if we begin in the lower-level state $s = 1/7$, we obtain the lower-level history $h = (1/7, 2/7, 4/7, 6/7, 2/7, 4/7, \dots)$. After coarse graining via σ, this becomes the higher-level history $(A, A, B, B, A, B, \dots)$. Let $\mathbb{\Omega}$ be the set of all higher-

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[22] For other, similar examples, see Winnie 1998, 310–14; Suppes 1999; and especially Werndl 2009.

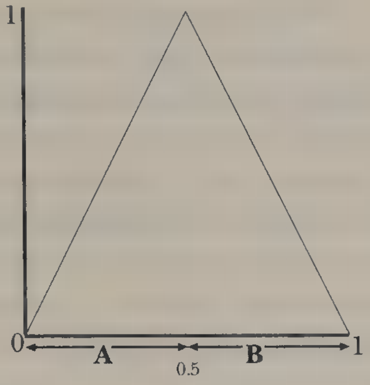

Figure 2. The transition rule f

level histories that can be obtained in this way. Then Ω contains *every* possible function from T into $\{A,B\}$. Thus, there is not only indeterminism in Ω, but "maximal" indeterminism: *every* truncated history up to time t can be extended to two truncated histories up to time $t + 1$, *four* truncated histories up to time $t + 2$, and so on.

To see how a nondegenerate chance structure on Ω arises in a very natural way, consider figure 3. Suppose a higher-level history $\boldsymbol{h}$ begins with A at time $t = 1$. Then $\boldsymbol{h}$ must be the coarsened counterpart of a lower-level history h beginning at some state s between 0 and ½. There are two possibilities: either $0 \leq s \leq$ ¼, or ¼ $< s \leq$ ½. In the first case, $f(s)$ (and thus $h(2)$) must be between 0 and ½, and so $\boldsymbol{h}(2) = A$. In the second case, $f(s)$ (and thus $h(2)$) must be between ½ and 1, and so $\boldsymbol{h}(2) = B$. Similarly, if $\boldsymbol{h}$ begins with B at time $t = 1$, its lower-level realizer must begin with some state between ½ and 1. Here, either ½ $< s <$ ¾, or ¾ $\leq s \leq$ 1. In the first case, $\boldsymbol{h}(2) = B$; in the second, $\boldsymbol{h}(2) = A$.

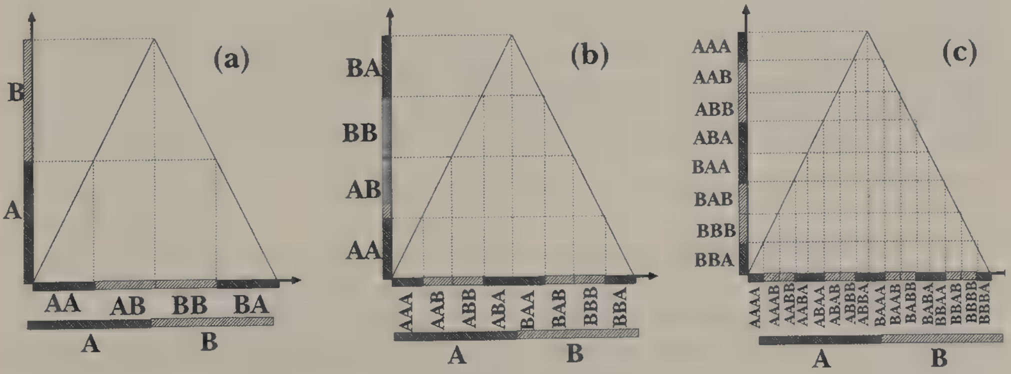

Figure 3. Higher-level histories generated by the function f and the partition {A, B}

So, depending on where exactly in the interval S the lower-level state falls at time $t = 1$, we obtain higher-level histories beginning with (A, A), (A, B), (B, B), or (B, A). These correspond exactly to four subintervals of S, each of length $1/4$, as shown in figure 3(a).

What happens at time $t = 3$? We must now consider *eight* subintervals of S, each of length $1/8$, as illustrated in figure 3(b). Which of these we start in determines the higher-level history up to time $t = 3$. For example, if $1/4 < s < 3/8$, then $1/2 < f(s) < 3/4$, and so $1/2 < f(f(s)) < 1$. Thus $\sigma(s) = A$, while $\sigma(f(s)) = B$ and $\sigma(f(f(s))) = B$. It follows that $\mathbb{h}(s)$ begins with (A, B, B).

To determine the higher-level history up to time $t = 4$, we must consider sixteen subintervals of S, each of length $1/16$. These correspond to the sixteen possible truncated histories of length 4, as illustrated in figure 3(c).

By iterating this argument, we see that, for any time t, the interval S can be subdivided into 2^t subintervals, each of length $1/2^t$, which correspond to the 2^t possible truncated histories of length t in Ω. This symmetry suggests a chance structure for the higher-level system, where each of these 2^t truncated histories has an equal chance of occurring. A higher-level history can then be seen as a sequence of random choices between A and B, both having probability $1/2$, and with different choices independent of one another, as in a sequence of fair coin tosses. In other words, the higher-level chance structure is that of a classic *Bernoulli process*. There is clearly no barrier for this chance structure to satisfy the six desiderata on objective chance.[23]

One might object that the emergence of nondegenerate objective chance in this example is an artifact of the excessively coarse partition of S into only two subintervals, from 0 to $1/2$ and from $1/2$ to 1, which we labeled A and B. But nondegenerate objective chance also emerges from finer partitions. Suppose, for example, we partition S into four subintervals of length $1/4$ each, labeled $\{AA, AB, BB, BA\}$, as in figure 3(a). Then an argument similar to the one just given shows that for any higher-level history $\mathbb{h}$ (now a function from T into $\mathbb{S} = \{AA, AB, BB, BA\}$), if $\mathbb{h}(t) = AA$

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[23] The set Ω of higher-level histories as formally defined here admits many other nondegenerate chance structures. The one we describe has some claim to being the most "natural" one, as it is invariant under an exchange of the symbols A and B; it can thus be motivated by some reasonable assumptions about the symmetries of the system. But it is not necessary for our purposes to show that this chance structure is in any way unique or canonical. What matters is that it is consistent with the six desiderata (at the level of Ω).

(for example), we must have $\mathbb{P}\mathrm{r}_{\mathbb{h},t}[\mathbb{h}(t+1) = AA] = 1/2$ and $\mathbb{P}\mathrm{r}_{\mathbb{h},t}[\mathbb{h}(t+1) = AB] = 1/2$. Similar points apply if $\mathbb{h}(t)$ is *AB*, *BB*, or *BA*. If, instead, we partition *S* into eight subintervals of length 1/8 each, labeled {*AAA*, *AAB*, *ABB*, *ABA*, *BAA*, *BAB*, *BBB*, *BBA*}, as in figure 3(b), then nondegenerate chance emerges again: for any higher-level history $\mathbb{h}$ (now over an even finer $\mathbb{S}$), if $\mathbb{h}(t) = BBB$ (for example), we have $\mathbb{P}\mathrm{r}_{\mathbb{h},t}[\mathbb{h}(t+1) = BBA] = 1/2$ and $\mathbb{P}\mathrm{r}_{\mathbb{h},t}[\mathbb{h}(t+1) = BBB] = 1/2$.

Indeed, a nondegenerate chance structure emerges for *any* finite partition of the interval *S*.[24] The reason is that lower-level histories of the system are extremely sensitive to small perturbations. To see this, suppose that s and s' are two points in the interval *S*, which generate lower-level histories h and h', corresponding to higher-level histories $\mathbb{h}$ and $\mathbb{h}'$ via some coarse-graining function σ. Suppose s and s' are very close together. The distance between $f(s)$ and $f(s')$ will then typically be *twice* the distance between s and s'.[25] And the distance between $f(f(s))$ and $f(f(s'))$ will typically be *twice* that between $f(s)$ and $f(s')$ (hence *four* times the distance between s and s'),[26] and so on. In this way, the lower-level histories h and h' will rapidly diverge from each other. This, in turn, will lead the corresponding higher-level histories $\mathbb{h}$ and $\mathbb{h}'$ to come apart eventually. Even if two higher-level histories $\mathbb{h}$ and $\mathbb{h}'$ agree for their first 2 million entries, there is no reason for $\mathbb{h}(2{,}000{,}001)$ to be the same as $\mathbb{h}'(2{,}000{,}001)$.

6. The Objective-Epistemic Distinction at Every Level

In section 3, we drew the distinction between objective chance and epistemic probability at the lower level of description (that is, in Ω). However, the *same* distinction can be drawn at the higher level (in $\mathbb{\Omega}$) and, indeed, at *every* level of description. Objective chance at any level is represented by whichever family of history-and-time-indexed probability functions "best" satisfies the six desiderata *at that level.* An epistemic probability function is only required to satisfy the epistemic probability-possibility desideratum at the relevant level and, if it is also constrained by level-specific chance information, the chance-credence desideratum. While an objective chance function can be nondegenerate *only if* there is indeterminism at the level at which the function is defined, an epistemic probability function can be nondegenerate even if there is determinism at that level.

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[24] See Werndl 2009, sec. 4.2, for similar observations.

[25] This is true as long as s and s' are both in subinterval *A*, or both in subinterval *B*.

[26] This is true as long as $f(s)$ and $f(s')$ are both in subinterval *A*, or both in subinterval *B*.

Importantly, our earlier operational test for *pure* epistemic probability applies at each level. A nondegenerate probability assignment at a given level meets the sufficient condition for being *purely epistemic*—not driven by any chance hypotheses—if it becomes degenerate once we conditionalize on complete information about the truncated history *at that level.*

> **Test for pure epistemic probability, where $\boldsymbol{\Omega}$ is the level-specific set of histories:** Let $\mathbb{P}\mathrm{r}$ be a probability function held by an agent A in history $\boldsymbol{h} \in \boldsymbol{\Omega}$ at time t, with $\mathbb{P}\mathrm{r}$ defined on $\mathcal{A}(\boldsymbol{\Omega})$, and suppose $\mathbb{P}\mathrm{r}(\mathbb{E})$ is nondegenerate for some event $\mathbb{E} \subseteq \boldsymbol{\Omega}$. A *sufficient condition* for $\mathbb{P}\mathrm{r}$ to be *purely epistemic* with respect to $\mathbb{E}$ is that $\mathbb{P}\mathrm{r}(\mathbb{E} \mid \boldsymbol{h}_t) = 0$ or 1. As before, $\mathbb{P}\mathrm{r}(\mathbb{E} \mid \boldsymbol{h}_t)$ stands for $\mathbb{P}\mathrm{r}(\mathbb{E} \mid \mathbb{I})$, where $\mathbb{I}$ is the information that the truncated history is $\boldsymbol{h}_t$; formally $\mathbb{I} = \{\boldsymbol{h}' \in \boldsymbol{\Omega}\colon \boldsymbol{h}'$ is a continuation of $\boldsymbol{h}_t\}$.

Consider the example of two dice being thrown onto a gaming table. Perhaps the system of tumbling dice admits a microphysical description Ω that is completely deterministic. However, as explained in section 5, the system may also admit a higher-level description $\boldsymbol{\Omega}$ in which the objective chance that the gambler is about to throw snake-eyes (a pair of ones) is $^1/_{36}$. Now suppose the gambler has already thrown the dice, but the result is hidden from your view by a barrier. The gambler can see the dice, but you cannot. There is no longer any nondegenerate objective chance here; either the dice came up snake-eyes, or they did not. The objective chance of this event is now either zero or one. But from *your* perspective, with limited information, the epistemic probability of the event (your credence) remains $^1/_{36}$. Once the barrier is removed, however, you will assign probability 0 or 1. In this story, there is both objective chance (about how the dice will land in the future) and epistemic probability (about how the dice have already landed).

For another example, consider the sort of uncertainty confronted by meteorologists. Perhaps the Earth's atmosphere admits a microphysical description Ω that is completely deterministic. However, this system also admits a higher-level description $\boldsymbol{\Omega}$, in which the objective chances of future weather events are nondegenerate. Meteorologists gather data from a large array of weather sensors (thermometers, hygrometers, barometers, and so forth) and analyze it with computers to predict tomorrow's weather. These predictions are uncertain, and this uncertainty arises in part from the existence of nondegenerate objective chance about tomorrow's weather *at the level of* $\mathcal{\Omega}$. Indeed, meteorologists model the weather as a stochastic system. However, meteorologists also confront another kind of uncertainty. Their network of sensors is sparse. The current meteorological conditions at some location X between two sensors are unknown. But the meteorologists can assign a probability distribution to the current conditions at X. This is a purely *epistemic* probability; if there *had* been a sensor at X, the epistemic probability for the conditions at X would be degenerate, because the meteorologists would know the actual conditions at X.

Finally, consider an example from the social world. The police in a big city wish to forecast crime rates in various neighborhoods, in order to organize effective patrols. Whether or not there is some physical or neuropsychological level (Ω) at which each individual crime is predetermined, at the ordinary human or social level ($\mathcal{\Omega}$), the police will have to treat patterns of crime as involving nondegenerate objective chance. The chance of various crimes happening will differ from neighborhood to neighborhood: there is a higher chance of petty theft and pickpocketing at the railway station than on a quiet residential street. The probabilities in question would not become degenerate even if the police had complete information about the human and social history up to now. Contrast this with a murder investigation in which the police assign probability ⅓ to the hypothesis that Jones did it. This probability is purely epistemic. Conditional on complete historical information (at the level of $\mathcal{\Omega}$), it would collapse into 0 or 1, since the relevant history would settle the matter.

Our claim that there is a well-defined distinction between objective chance and epistemic probability at every level of description and that nondegenerate objective chance can be a level-specific phenomenon does not depend on our reasons for employing descriptions at different levels. In particular, the question of *why* we should describe a system at a particular level—say, a higher level—is distinct from the question of whether, *at that level*, the system admits nondegenerate objective chance. Recall that *once we are describing a system at a given level*, the level-specific chance facts are represented by whichever family of history-and-time-indexed probability functions best satisfies the six desiderata *at that level*. What makes them objective chance functions, *relative to that level*, is simply the satisfaction of the six desiderata. Their status as level-specific objective chance functions does not depend on any claims about the "objectivity" of the level itself, and indeed we make no such claims.

Keep in mind that the identified functions are not interpreted as representing "objective chance *simpliciter*," independently of the level of description. They represent objective chance *relative to that level.*

On the present picture, one could consistently hold that (i) higher-level descriptions are needed because of our informational (or, more broadly, cognitive) limitations, and yet that (ii) *relative to the higher level,* there can be nondegenerate *objective* chance as distinct from *epistemic* probability. It is a consequence of what we have argued that even if our reasons for employing higher-level descriptions were *entirely* epistemic, this would not undermine the distinction between objective chance and epistemic probability *relative to that level*; that distinction is drawn solely on the basis of level-specific criteria.[27]

7. The Reasons for Employing Higher-Level Descriptions

Although the *well-definedness* of the distinction between higher-level chance and higher-level epistemic probability does not depend on our reasons for employing higher-level descriptions, its *interest-value* does. If higher-level descriptions could easily be dispensed with, then the claim that there can be nondegenerate objective chance, relative to the higher level, would be, at most, an idle theoretical curiosity. However, if higher-level descriptions are indispensable in practice, then higher-level chance is a phenomenon of theoretical as well as practical interest. In what follows, we briefly review some of the reasons for employing higher-level descriptions and suggest that, in the case of many systems, such descriptions may actually be indispensable. Of course, a full defense of this claim—familiar from many discussions of the special sciences—is beyond the scope of this essay. But it is also not required for the theory of higher-level chance that we have developed. The point of this section is merely to summarize, in broad outline, why higher-level descriptions are needed in many contexts. We have already presented the case for the possibility of level-specific chance itself.

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[27] Our point that there can be objective chance relative to a given level of description, independently of our reasons for employing descriptions at that level, is reminiscent of a point that Cohen and Callender (2009) make about (in our terminology) level-relative laws. They argue that relativizing laws to levels "does not entail subjectivity in the sense of making scientists (or others) invulnerable to errors about the laws" (ibid., 30). The reason is that there are observer-independent facts about which generalizations satisfy the appropriate level-relative criteria for lawhood (ibid.). Similarly, we suggest that the question of whether a given family of history-and-time-indexed probability functions satisfies the desiderata for objective chance *at a particular level* does not depend on the observer or on why he or she is interested in that level.

The simplest reason for using higher-level descriptions is that we often lack, and cannot acquire, complete information about the lower-level state of a system. We have seen that even if this were the only reason for using higher-level descriptions, it would still not undermine the distinction between objective chance and epistemic probability, *relative to the higher level.* However, incomplete information about a system's lower-level state is not the only reason for using higher-level descriptions. Arguably, whether or not we could acquire complete information about the lower-level state, higher-level descriptions are often necessary for agents such as ourselves to avoid the unmanageable complexity of the lower level.

First, the lower-level dynamics of many systems are *chaotic.* Very small errors in our measurement of the current state of a system can lead to very large errors in our predictions of the system's future behavior. (A simple example is the system shown in figures 2 and 3 in section 5.) Since some tiny amount of measurement error is inevitable in practice, prediction may not generally be feasible at the lower level. By contrast, under a suitable coarse graining, the chaotically diverging trajectories at the lower level can perhaps be amalgamated into a single, predictable trajectory at some higher level—or at least, into a higher-level stochastic process with a manageable amount of randomness; weather forecasting is an example.

Second, even if we could make *perfect* measurements, *or* even if the lower-level dynamics were *not* chaotic, lower-level predictions might still be uneconomical or unparsimonious due to computational complexity and often not what we are ultimately interested in. For example, imagine that we pour a few drops of blue dye into one part of a water tank, undisturbed by any movement. How will the dye diffuse? If viewed through a microscope, each of the trillions of jostling, jiggling blue dye particles would exhibit Brownian motion and wander along some convoluted, labyrinthine path through the tank, which, in turn, is the result of a deterministic kinetic-molecular process. All this is extremely hard to model.[28] At a macroscopic level, however, the system admits a very simple and informative description: if we write down a function describing the three-dimensional density distribution of the dye in the water at time zero, then this function evolves predictably under a partial differential equation called the *heat equation*, which is often amenable to a relatively easy computational solution at all future times.[29] Similarly, to give a more informal example, the dynamics of trillions of molecules of water and other organic compounds ricocheting around a tea cup are hard to model at a microphysical level, while what we are ultimately interested in is how long it takes for the tea to brew or how strong it is, for which we have simple rules of thumb. The examples illustrate that it is often more economical, parsimonious, and informative to use a coarse-grained model, perhaps a statistical one, at a higher level of description.[30]

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[28] Note further that the apparent "randomness" of each dye particle's Brownian motion (though not our focus here) is arguably yet another instance of emergent chance: it is due to the way each dye particle is constantly buffeted by millions of much smaller and faster water molecules. At the microscope's level of resolution, a dye particle's motion is best described by an elegant probabilistic model called the *Wiener process.* But it supervenes on the deterministic dynamics of the underlying, more fine-grained molecular collisions.

Third, in many systems, there are robust regularities among higher-level properties. We have just mentioned two such regularities, in the heat equation governing diffusion processes and in the familiar rules of thumb telling us how long it takes for a tea to brew. Further, proponents of "higher-level causation" defend the view that the causes of higher-level effects are sometimes other higher-level properties and not always the token realizers at the lower level (see, for example, the contributions in Ellis, Noble, and O'Connor 2012). This view is particularly plausible when (as common in the special sciences) causation is understood as difference making (List and Menzies 2009, Raatikainen 2010). The difference-making cause of an agent's action, for example, may be, not his or her brain state, microphysically described, but his or her mental state, described at a higher level. Similarly, the difference-making cause of an increase in inflation may be a set of actions of the central bank and the government, together with other macroeconomic properties, all described at a higher level, rather than their token realizers at the individual level, let alone at the microphysical one (for example, Sawyer 2003, List and Spiekermann 2013). If these claims are correct, then higher-level descriptions sometimes yield better causal explanations than lower-level ones. Indeed, the special sciences have taught us that, for many phenomena, the most explanatorily illuminating level of description is not a microphysical one, but a chemical, biological, psychological, or social one.[31]

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[29] For instance, if the concentration at time zero is a multivariate standard normal distribution with variance σ^2, then the concentration at time t will be a multivariate standard normal distribution with variance $Ct + \sigma^2$, where C is a constant describing the diffusion speed, determined by the water temperature, the mass of the dye particles, and so on.

[30] The case for using higher-level statistical models is frequently made in the literature on deterministic chance (for example, Loewer 2001, Frigg and Hoefer 2010, Albert 2012).

Furthermore, although we are often interested in higher-level information—both in ordinary life and in the special sciences—recovering this information from a complete lower-level description of a system is sometimes not merely uneconomical but not possible at all, given our cognitive limitations. The "coarse-graining" map σ may be a well-defined mathematical object, but there is no reason to assume that it admits a simple (or even just finite) description in any formal language available to us. Via σ, each higher-level history h corresponds to an equivalence class H of lower-level histories. Unfortunately, the simplest description of H may be just an enumeration of its elements. If H contains infinitely many elements, it may not even be describable by *any* finite sentence. This is not an outlandish possibility; there is a sense in which, if Ω is itself infinite, "almost all" subsets of Ω admit no finite description.[32] And even if H *is* finitely describable, the shortest description of H could be astronomically large: it may contain as many symbols as there are atoms in the Milky Way galaxy.

Finally, and more speculatively, even if the description of H is finite and of manageable length, it is still possible that it is *formally undecidable*,[33] or *computationally intractable*,[34] whether any particular lower-level history belongs to H or not. If this is the case, then some questions about a system's higher-level history cannot be answered on the basis of a lower-level description of the system alone, even a complete description.[35] Instead, we may need to use higher-level descriptions as "primitives."

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[31] For a related discussion, see Callender and Cohen (2010, 427), who argue that "although all events supervene on a fundamental level, there is no one unique locus of projectibility; rather there are a large number of loci corresponding to the different areas (ecology, economics, solid-state chemistry, etc.) in which there are simple and strong generalizations to be made."

[32] The class of all subsets of Ω that admit a finite description is *countable*. But the class of *all* subsets of Ω is *uncountable*. So the former is a very small subclass of the latter.

[33] This means that there is no algorithm that, given a precise specification of the low-level history h, will always correctly determine whether h is in H, after a finite computational process. Many apparently "simple" questions of the form "Is object a of type B?" are formally undecidable. For example: Given an initial point in an orbit of a dynamical system, will this orbit enter a certain region of the state space? Given an arbitrary finite collection of polygonal tiles, can we tile the Euclidean plane with nonoverlapping copies of them? Given a geometric object obtained by gluing together polygons, polyhedrons, and their higher-dimensional counterparts (a *simplicial complex*), does this object have a hole in it like a donut? (Is it *simply connected*?) The claim is not that *no* problems of these types can be solved. For many special cases, there are solutions. The claim is that there is no general algorithm for *all* problems of these types. For an introduction to formal undecidability and other examples, see Moore and Mertens 2011, chap. 7.

In sum, there are both practical and theoretical reasons why higher-level descriptions are often indispensable. As should be evident, the present arguments are in line with familiar arguments against the reducibility of higher-level properties to lower-level ones (among other things, due to multiple realizability, as argued, for example, in Fodor 1974 and Putnam 1975), and for nonreductive physicalism in the special sciences (for example, Jackson and Pettit 1990, Pereboom 2002, List and Menzies 2009). Nonreductive physicalism is the view that higher-level properties (i) supervene on lower-level properties, (ii) are nonidentical to them (and admit no type reduction), and (iii) play a causal role in the world. Our case for the need to employ higher-level descriptions can be regarded as an instance of the case for nonreductive physicalism more generally.

8. An Objection

We have seen that different levels of description of a system correspond to different algebras of events on which probabilities are defined and that the distinction between objective chance and epistemic probability can be drawn at every level. A necessary condition for the occurrence of nondegenerate chance at a given level is indeterminism *at that level*. Since higher-level indeterminism is consistent with lower-level determinism, the latter does not rule out nondegenerate chance at a higher level.

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[34] Informally, this means that although there *is* an algorithm to determine whether h is in H, it may take trillions of years for it to produce an answer. Thus it is unknowable, *in practice*, whether h is in H. The best-known (but not the only) formal notion of computational intractability is *NP-completeness*. Many apparently "simple" problems are NP-complete. For example: Given an arbitrary expression in propositional logic, can we assign truth-values to the propositional letters to make it true? Given a finite set of points $\{\mathbf{p}, \mathbf{q}, \mathbf{r}, \mathbf{s}, \mathbf{t}, \ldots\}$ in Euclidean space, is $\mathbf{p}$ a weighted average of $\mathbf{q}, \mathbf{r}, \mathbf{s}, \mathbf{t}, \ldots$? Given a list of numbers, can we split it into two sublists that add up to the same value? Given an arbitrary network of vertices and links, is there a path that goes through every vertex exactly once? NP-complete problems are only the *bottom* of an infinite hierarchy of increasingly intractable (but decidable) problems. See Moore and Mertens 2011, chaps. 5–6.

[35] By contrast, given the solution to one formally undecidable problem, we can solve any other formally undecidable problem at the same level of the relevant hierarchy; given the solution to one NP-complete problem, we can solve any other NP-hard problem relatively easily, via a "polynomial-time reduction" (Moore and Mertens 2011, secs. 7.2.4 and 5.1).

A critic might still object that when there is lower-level determinism, such higher-level chance is not "true" objective chance. "True" objective chance, the critic might say, requires indeterminism at the lowest or most fundamental level of description. This is what Schaffer (2007) seems to suggest. So formulated, the objection makes the assumption that there is a lowest or most fundamental level, which can be challenged.[36] But there is a second version of the objection, which does not make this assumption. It asserts that if there is *some* level at which the system is deterministic, then any nondegenerate probability at a higher level can only be epistemic. Or relatedly: if there is nondegenerate objective chance at a higher level, then all lower levels must also admit nondegenerate objective chance.[37] What can be said in response to this objection?

First, the objection, in both versions, overlooks the level-specific nature of the distinction between objective chance and epistemic probability. On the picture we have defended, the hallmark of an objective chance structure at a particular level is the satisfaction of the six desiderata *at that level*, not any facts about other levels. As our formal results have shown, nondegenerate objective chance, understood in this level-specific way, requires indeterminism at the same level, not at others.

Second, in the absence of indeterminism "all the way down," the critic endorsing the objection is compelled to lump all higher-level probability together into an "epistemic" (as opposed to "objective") category. More precisely, the critic is committed to the view that if there is determinism at either the fundamental level (for the first version of the objection) or some level below the level of interest (for the second), then any higher-level probability can only be epistemic. Objective chance at the higher level would have to be accompanied by indeterminism at the lower level. For example, if a system of tumbling dice has deterministic microphysical foundations, the critic loses the ability to distinguish between the *chance* of the dice coming up snake-eyes in the next round (a future event that is "random" from a higher-level perspective) and our *credence* in the hypothesis that, in the last round, the outcome was snake-eyes (a past event that is already settled but that we have not yet observed). The second of these probabilities would become degenerate if we received complete information about the truncated higher-level history up to now. The first probability would not. Indeed, it would satisfy the desiderata for playing the "chance role" at the higher level. Our distinction between higher-level chance and higher-level epistemic probability captures this point naturally. The critic is unable to make the distinction.

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[36] For example, in an earlier article, Schaffer (2003, 498) argues that there is "no evidence" in support of that assumption and that there could plausibly be "an infinite descending hierarchy of levels" without any bottom (ibid., 499). In subsequent work, however, Schaffer implies that there is a fundamental level (see especially Schaffer 2010, 37).

[37] We are grateful to the editors for helping us sharpen this objection.

9. The Mutual Embeddability of Deterministic and Indeterministic Systems

Two final results should give further pause to those who think that we can draw a single distinction between objective chance and epistemic probability *simpliciter*, or between indeterminism and determinism *simpliciter*, instead of drawing a family of level-specific such distinctions.

> **Observation 4:** *Any* deterministic system can be expressed as emerging from a more fine-grained indeterministic system.

> **Observation 5:** *Any* indeterministic system can be expressed as emerging from a more fine-grained deterministic system.

Observation 4 shows that no matter how fine-grained the level is at which a given system displays determinism, we can never rule out the possibility of indeterminism at an even lower level. Similarly, Observation 5 shows that no matter how fine-grained the level is at which a given system displays indeterminism (and admits nondegenerate chance), we can never rule out the possibility of determinism at an even lower level.[38]

To prove Observation 4, let T be any set of times, $\mathbb{S}$ any state space, and $\mathbb{\Omega}$ any set of deterministic histories (that is, functions from T into $\mathbb{S}$). Now let $S = \mathbb{S} \times \{0, 1\}$. In other words, S is the set of all ordered pairs of the form $(\mathbb{s}, b)$, where $\mathbb{s}$ is an element of $\mathbb{S}$, and b is either 0 or 1. Any S-valued history (that is, any function h from T into S) is thus a combination of two functions: a function $\mathbb{h}$ from T into $\mathbb{S}$, and a function β from T into $\{0, 1\}$. Let Ω be the set of all histories $(\mathbb{h}, \beta)$, where $\mathbb{h}$ is any element of $\mathbb{\Omega}$, and β is any possible function from T into $\{0, 1\}$. It is clear that Ω is a set of *indeterministic* histories; any length-t truncated history $(\mathbb{h}_t, \beta_t)$ in Ω has two possible extensions to a truncated history of length $t + 1$: one where $\beta(t+1) = 0$ and one where $\beta(t+1) = 1$. Now we define the function σ from S to $\mathbb{S}$ by setting $\sigma(\mathbb{s}, b) = \mathbb{s}$ for any $\mathbb{s}$ in $\mathbb{S}$ and b in $\{0, 1\}$. Then σ is a coarse-graining map that converts the (indeterministic) histories of Ω into the (deterministic) histories of $\mathbb{\Omega}$.

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[38] See Winnie 1998, 305–6, and Werndl 2009, sec. 3.2, for results similar to Observation 5. See also Suppes 1993, 250–52 and 254, for an earlier discussion.

To prove Observation 5, let T be any set of times, $\mathbb{S}$ any state space, and $\mathbb{\Omega}$ any set of indeterministic histories (that is, functions from T into $\mathbb{S}$). Now define $S = \mathbb{\Omega} \times T$. In other words, S is the set of all ordered pairs of the form $(\mathbb{h}, t)$, where $\mathbb{h}$ is any element of $\mathbb{\Omega}$ and t is some point in time. For any history $\mathbb{h}$ in $\mathbb{\Omega}$, we define a function h from T into S by setting $h(t) = (\mathbb{h}, t)$, for all t in T. Clearly, this is a completely deterministic history. (Heuristically, the lower-level world consists of a "book" and a "clock." The "book" is a complete record of the entire history of the higher-level world, both past and future. It is represented by $\mathbb{h}$, and it never changes. The "clock" is represented by the t-coordinate, which simply records the current time, and thus evolves in an entirely predictable way.) Let Ω be the set of all lower-level histories obtained in this way; then Ω is a deterministic system. Finally, we define a function σ from S to $\mathbb{S}$ by setting $\sigma(\mathbb{h}, t) = \mathbb{h}(t)$, for any $\mathbb{h}$ in $\mathbb{\Omega}$ and t in T. Then σ is a coarse-graining map that converts the (deterministic) histories of Ω into the (indeterministic) histories of $\mathbb{\Omega}$.

Of course, these are purely mathematical constructions, which only provide a proof of possibility. We do not claim that the lower level of any physical system would have the structure described in the previous two paragraphs. In reality, the lower level would presumably be some system of interacting particles and fields, of the kind described in modern microphysical theories. But these examples illustrate that there is no necessary entailment from indeterminism at a higher level to indeterminism at a lower level, or vice versa. Similarly, there is no reason to assume that when a system admits a nondegenerate chance structure at a higher level, it must also admit a nondegenerate chance structure at a lower level, or vice versa.

Furthermore, the present constructions can be iterated indefinitely; it is perfectly possible to have a deterministic higher-level system that is a coarse graining of an indeterministic lower-level system, which is in turn a coarse graining of an even *lower*-level deterministic system, and so on. There could be an *infinite* hierarchy of such systems, with no "rock bottom" level; it could be "turtles all the way down."[39]

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[39] As mentioned above, Schaffer (2003) argues that an infinite hierarchy of levels without any bottom is a coherent possibility, though he does not examine the question of

> **Observation 6:** An infinite hierarchy of levels, forever alternating between deterministic and indeterministic sets of level-specific histories, is a coherent (and unfalsifiable) possibility.[40]

In this scenario, it would make no sense to ask whether the system was deterministic or indeterministic "*simpliciter.*" There would be no level-independent answer to this question.

10. Conclusion

Objective chance, along with indeterminism, should be understood as a level-specific phenomenon, which stands in no conflict with determinism at other levels, both lower and higher.[41] Objective chance is only incompatible with determinism *at the same level.* There is not a single distinction between objective chance and epistemic probability but a separate distinction for each level.

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determinism in such a setting. It is worth noting that the metaphysical picture in Schaffer 2003 supports the reality of higher-level properties. He criticizes the "ontological attitude according to which the entities [for our purposes: properties] of the fundamental level are *primarily* real, while any remaining contingent entities [properties] are at best derivative, if real at all" (ibid., 498). He suggests that if we drop the assumption of a fundamental level while retaining "a hierarchical picture of nature as stratified into *levels*" (a picture he takes to be "reflected in the structure and discoveries of the sciences"), we arrive at "a far more palatable metaphysic in which . . . all entities [for us: properties] are equally real" (ibid.).

[40] Mathematically, this can be obtained through an *inverse limit* construction.

[41] Throughout this essay, our use of the conventional terminology of "levels" of description should not be taken to imply that there must generally exist a *linearly ordered* hierarchy of levels. Our approach of representing different levels of description in terms of different algebras of events, related to one another via coarse graining, permits the existence of an entire *lattice* of "levels," *partially ordered* by the coarse-graining relation.

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Matthew Stuart, *Locke's Metaphysics.*
Oxford: Oxford University Press, 2013. xi +522 pp.

Matthew Stuart has written a very good book. In ambition, scope, and attention to text, it most resembles Michael Ayers's *Locke: Epistemology and Ontology.* Ayers's book is over twenty years old. Stuart's book is a worthy successor, and, in some respects, a worthy replacement.

Stuart sets for himself the mission of describing every part of Locke's metaphysics. He isn't especially concerned with placing these doctrines in their scientific or theological contexts, and, as a result, the book has a certain focus and fullness. Its hallmark is the use of philosophical sophistication and subtlety to show that passages that seem for all the world to be incompatible are actually consistent. He does this so well that I found it often hard to criticize his readings, even where I prefer another one.

For the most part, Stuart avoids evaluating Locke's positions, though his underlying agreement with them often shines through. An exotic exception is in his discussion of Locke on the infinitude of space, where he anachronistically chides him for not considering the possibility that space has a non-Euclidean structure, and motion in a straight line takes you back to where you are (67). A more important exception follows his discussion of Locke on personal identity. A standard response to Reid's brave officer objection is to extend personal identity beyond the strict bounds of memory by either appealing to the transitivity of identity or to Locke's hint that the mere potential for memory might suffice. Stuart abjures such trickery. On his reading, a Lockean person extends only so far as his or her awareness does, and forgetfulness rules out sameness of person. The result is something akin to a seventeenth-century version of *Dasein,* a person of pure consciousness. In section 54, Stuart fends off objections that show the peculiar character of Locke's concept of person before finally letting the force of reason persuade him that the concept won't work for some of the judicial, theological, and practical purposes that Locke sets for it. It might be useful for other purposes, however.

I presented some of this material at an Author Meets Critics session at the 2014 meeting of the Central APA. I'm grateful to Matthew for setting me straight on a misunderstanding, to Antonia LoLordo for helpful comments, and to Jan Cover for a useful conversation.

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Most of Stuart's philosophical cleverness goes toward arguing for the consistency and plausibility of Lockean positions that might seem muddled to a less sympathetic eye. Here it's worth singling out his development of the concept of a degenerate power, according to which a thing has a power to do such and such just in case it's actually doing such and such. Stuart offers this conceptual analysis as a way of making Locke's definition of secondary qualities as powers to produce ideas in us compatible with his claims that porphyry has no color in the dark and his claim that gold would cease to be yellow in a vacuum, but the analysis is interesting in its own right.

Stuart neglects very few texts. Where a passage makes trouble for his reading, he engages it and tries to limit the damage. Where taking a passage seriously makes the interpretive issues complicated, he embraces the complications. For example, Locke stipulates that by 'preference', he doesn't really mean preference, but rather volition. This makes it hard to understand certain famous examples of Locke's, including the person who stays voluntarily in a locked room and the person who falls from a bridge involuntarily. Instead of downplaying the stipulation, Stuart rolls up his sleeves on the way to developing an account of Locke as someone who held a volitionist theory of action. His treatment of the *Essay*'s argument for the existence of God is also precise and fine grained, treating with care interesting passages that one might miss by reading too quickly or with preconceptions.

Peter Geach, Bruce Langtry, and others have read Locke as a relative identity theorist, but Stuart has worked out the details and he has taken the textual case much further than his predecessors. Stuart argues that Locke's "relativism about identity fits comfortably with his rejection of essentialism" (328), which is a bit vague. Clearly, one can reject *de re* modality and also reject relative identity, as the example of Quine shows. Whether one can believe in both *de re* modality and relative identity depends on the version of relative identity. Stuart's Locke thinks that we can judge whether something continues to exist only relative to a sort, and so we can judge whether a feature belongs to it necessarily only relative to a sort.

For Stuart, the question of whether Locke is a relative identity theorist is equivalent to the question of whether he believes that it's ever the case that "*x* can be the same F as *y* without being the same G as *y*, even if *x* and *y* are both Gs" (348). Since it's plain that Locke believes that after the prince and the cobbler switch souls that the same man remains in the body while the same person does not, this question is roughly equivalent to the question of whether Locke takes 'person' and 'man' to be predicated of the same thing. Much of Stuart's firepower is directed against the standard view of Vere Chappell's, according to which Locke believes in the numerical distinctness of *persons, men*, and *masses of matter.*

While treating Locke's discussion of annihilation, Stuart allows that in such contexts we may take Locke to accept "body" and "finite immaterial substance" as substance concepts and treat every other concept as a phase sortal. By making this concession, Stuart isn't making bodies and souls into primary beings upon which all the rest depend, since he denies that Locke has the conceptual apparatus needed to make identity claims over time about bodies and immaterial thinking substances (337).

I'm not convinced by Stuart's interpretation of Locke on identity. I think that Locke allows himself the use of the concept of identity without qualification, and I'm not convinced that he rejects the copresence of elm trees and mass of matter as distinct entities at the same place and the same time. Still, I believe that other people will be convinced, and I believe that Stuart's reading is an impressive interpretive and philosophical achievement.

When the evidence doesn't take Stuart in any particular direction, he lays the options out before the reader. He sets out five possible options for what Locke might have meant when he says that ideas of primary qualities resemble something in bodies before concluding that whatever he meant, it was something that wouldn't have surprised his readers. He runs through four readings of Locke on substance, settling in the end on the view that Locke wasn't happy about our idea of substance, but didn't have any definite ideas about how to clarify things.

Stuart's project of giving a full account of everything that Locke says about metaphysics costs a bit of coherence. The book isn't structured around defending and developing a single thesis. The last two chapters on Locke's account of agency feel tacked on. But by way of compensation, Stuart does give us a full treatment of Locke's metaphysics, and the reader can dip in where he or she pleases. Each chapter begins with a useful list of theses belonging to the subsections, and the book is clearly written, so there's not much danger in getting lost.

Stuart has written a big, judicious, reliable book. Where it diverges from standard readings of Locke's metaphysics, he has good textual support backing him up. I recommend it for anyone interested in Locke's metaphysics and for historically minded metaphysicians interested in powers, relative identity, and volitional accounts of agency.

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Lynne Rudder Baker, *Naturalism and the First-Person Perspective.*
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Many contemporary philosophers hold a version of scientific naturalism, the view according to which (i) scientific ontology is all the ontology there is and (ii) the methods of science are the only legitimate ways of knowing (and consequently all other alleged ways of knowing are either illusory or reducible in principle to the scientific ones). A corollary of this view is that philosophy is continuous with science with respect to subject matter, method, and purposes.

As shown by Lynne Baker in her profound new book, scientific naturalism comes in different versions, depending on how its advocates respond to some crucial open issues. In particular, some of them (such as Philip Pettit) claim, and others (such as Hilary Kornblith and Philip Kitcher) deny, that all sciences are reducible to microphysics. And some (the "disenchanted naturalists," such as Alex Rosenberg) maintain that the so-called fundamental questions of life disintegrate once they are framed within the scientific worldview, while others (the "optimistic naturalists," such as Philip Kitcher and Daniel Dennett) think that such questions are legitimate and can be understood (if not answered) with scientifically kosher conceptual tools. However, all advocates of scientific naturalism encounter serious difficulties when they try to naturalize—either by reduction or elimination—the most relevant features of the commonsense view of the world. In this regard, Huw Price (2004, 74) has talked of a "Placement Problem": "If all reality is ultimately natural reality, how are we to 'place' moral facts, mathematical facts, meaning facts, and so on? How are we to locate topics of these kinds within a naturalistic framework, thus conceived?"

A different route has been taken by influential philosophers such as P. F. Strawson, John McDowell, Jennifer Hornsby, Hilary Putnam, and Barry Stroud, who have proposed different versions of a more liberal naturalism (for a general discussion, see De Caro and Macarthur 2010). These authors aim at accounting for the commonsense features of the world taken at face value, without being at odds with the scientific view of the world. The discussions between scientific naturalists and liberal naturalists—both at the metaphilosophical level and in regard to the possibility of naturalizing specific items—are arguably among the most stimulating in contemporary philosophy. *Naturalism and the First-Person Perspective* will greatly advance some of these discussions.

Baker sets a series of ambitious goals for her book. First of all, she sides with the liberal naturalists in claiming that what escapes naturalization is not necessarily ontologically unacceptable. When a phenomenon that is central in our lives appears impossible from the point of view of a particular philosophical conception, this is a kind of *reductio* for that conception: "We should not embrace a metaphysics that makes mundane but significant phenomena unintelligible" (73). Among the significant and arguably irreducible phenomena one cannot dispense with, there is a very important one that according to Baker has been unjustly neglected by both scientific and liberal naturalists: the first-person perspective of the world. According to her, genuinely first-person aspects of reality exist and they cannot be explained or explained away by science. This, however, is not because science adopts a third-person perspective, as is commonly thought, but rather because "the so-called third-person perspective is centerless; it is [Thomas Nagel's] 'view from nowhere'" (xix).

In this respect, Baker makes an important distinction between the "rudimentary first-person perspective" and the "robust first-person perspective" (a distinction that is very promising, it could be argued, since it appears confirmed by massive evidence coming out of cognitive science). The rudimentary first-person perspective is a dispositional property that does not require language, allows phenomenal consciousness, and makes it possible for an organism to interact, consciously and intentionally, with the environment. The robust first-person perspective, which subsumes the rudimentary one, is the capacity that every person endowed with a language has of thinking of himself or herself as the object (in a special sense) of his or her own thought. This capacity is a dispositional property, which is expressed with I* thoughts—that is, "every thought, utterance, or action that exhibits self-consciousness" (xx), such as "I hope that I* will be able to write a fair review of Lynne Baker's book." According to Baker, the robust first-person perspective is an emergent property that may globally supervene on the physical properties of the world, but can be neither explained by science nor explained away. Consequently, the account of reality advocated by scientific naturalism, which is wholly impersonal, must be false.

According to Baker, having a robust first-person perspective is indispensable for self-evaluation, self-understanding, moral responsibility, agency, practical reasoning, deliberation, and self-consciousness. On the last issue, Baker strongly disagrees with philosophers such as Ned Block and David Chalmers (but she could have also mentioned W. V. Quine or David Lewis) who do not find it scientifically or metaphysically puzzling.

Baker also defends a detailed non-Cartesian account of the first-person perspective, intended as an irreducible but not supernatural feature of reality. Her defense is based on two "unpopular views" (220), ontological emergence and downward causation. Against the mainstream, she argues that higher-level properties do not locally supervene on lower-level properties but are constituted by them—in the technical sense of "constitution" that Baker has explored at length in her past work. Still, she notices, property constitution is compatible with global supervenience (and this may make her views less alarming for some philosophers).

Finally, Baker advocates "near-naturalism," a view that in her opinion can adequately account for the first-person perspective. Adapting Dan Dennett's famous phrase, one could say that for Baker near naturalism can give what is worth wanting in naturalism without committing us to the ineffective reduction and elimination strategies of the commonsense features of the world. In fact, on the one side, near-naturalism "does not take science to be the exclusive arbiter of reality" (208); on the other side, it is not committed to supernaturalism. Therefore, Baker seems to have a point when she claims that near-naturalism is palatable for liberal naturalists. But one thing should be noted: the suffix "near" in the term signals the fact that this view is neutral regarding the possible existence of supernatural entities. Even naturalists of a liberal tendency (but not all: see, for example, Robert Audi [2000]) would probably disagree with such neutrality; but this does not change the fact that they could be happy with the positive part of Baker's conception.

Other philosophers, such as John McDowell, Akeel Bilgrami, David Macarthur, and Stephen White—who reject scientific naturalism but are also unhappy with supernaturalism—would disagree with Baker for another reason. These philosophers (either out of sympathy with a Wittgensteinian quietism or for more substantial reasons) tend to be skeptical about metaphysical accounts of such topics as the relation between physical and personal entities. Baker, by contrast, aims at working out the framework of a unified metaphysical view of the world that could encompass both scientific and commonsense entities. It is indeed an open question whether liberal naturalists should prefer a quietist or a metaphysical approach—and certainly one that will be debated for many years.

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Linda Zagzebski, *Epistemic Authority: A Theory of Trust, Authority, and Autonomy in Belief.*
Oxford: Oxford University Press, 2012. xiii +279 pp.

It is an exceptionally rare book that can be both a cutting-edge work on recently hot topics in epistemology—for example, rationality, disagreement, trust, and testimony—and also, under the surface, the latest defense of Catholicism in its centuries-long dispute with Protestantism over fundamental epistemic principles. Linda Zagzebski's highly ambitious book on epistemic authority manages to be both.

The ultimate aim is to vindicate epistemic authority as a crucial and unavoidable aspect of our intellectual lives—where this includes the moral and religious dimensions of our lives, as well as our more narrowly theoretical concerns. This is not a commonly accepted position in current epistemology, so Zagzebski begins by attempting to ground the acceptance of epistemic authority in epistemic autonomy and an individual's self-trust. In the first step of this project, she follows Richard Foley and William Alston in arguing that there is no noncircular way to show that our cognitive faculties reliably produce true beliefs. Foley and Alston think that our recognition of this circularity forces us to seek refuge in self-trust: we cannot know that our faculties are reliable, but we must use them anyway. For Zagzebski, however, reliance on self-trust is not a matter of settling for an inferior position. Rather, the argument from circularity is useful insofar as it reveals our *natural* self-trust.

Zagzebski's optimistic reading of our epistemic predicament is grounded in her account of rationality. She holds that "to be rational is to do a better job of what we do in any case—what our faculties do naturally" (30). If it is natural for us to desire the truth and to trust that our faculties are capable of allowing us to discover it, then it is rational for us to trust our faculties and to use them conscientiously. In addition to the natural desire for truth, we also "naturally desire and attempt to achieve a harmonious self" (31). This means that we try to resolve various forms of dissonance, which arise when there is some conflict to be found among the various components of the self: our beliefs, desires, and other attitudes, our decisions, and our emotions. Conscientious reflection is an extension of the natural psychological processes that lead to harmony in the self. And so, because "there is a connection between the natural and the normative" (33), what it is rational for us to believe and do and feel will be what survives conscientious reflection.

It is worth noting that this conception of rationality is more subjective than one might have expected, notwithstanding its grounding in the natural. In Zagzebski's (1996) earlier work on virtue epistemology, knowledge is taken to arise from acts of intellectual virtue, where it is essential to acts of virtue that they are successful in attaining their goal of producing true belief. Given that picture, one might expect to see epistemic rationality characterized as resulting from the exercise of the intellectual virtues. Instead, her focus in the present book is on conscientious reflection, where the link between such reflection and attaining the truth is largely set aside. On the resulting view, then, epistemic rationality is apparently divorced from knowledge and justification. Epistemic authority can give us reasons that make our beliefs rational, in some sense, but it is unclear whether those reasons can also permit us to know the truth.

After identifying the self-trust that is both natural and rational to each of us individually, Zagzebski argues that I am rationally committed to extending trust to others. This is so because I believe "that they are like me and that I should treat like cases alike" (61). If I trust my own conscientious efforts to reach the truth, then I should trust the similarly conscientious efforts of others. This does not mean that I should merely treat the beliefs of others as evidence to be weighed against other sources of evidence I might have. In cases where I trust the way someone else forms his belief more than I trust my own (isolated) attempts, "the conscientious thing to do is to let the other person stand in for me in my attempt to get the truth in that domain and to adopt his belief" (105). The other person is an authority for me; he generates a preemptive reason—that is, "a reason that replaces other reasons" I have (102)—for the belief in question. The authority of another person's belief, though preemptive for me, is nevertheless grounded in my own conscientious reflection. It is justified either by my conscientious judgment that I am more likely to get a true belief if I accept what the authority believes (110) or by my conscientious judgment that I am more likely to get a belief that survives my conscientious self-reflection if I accept what the authority believes (111) than if I try to figure out what to believe on my own.

This conception of epistemic authority fits naturally with the interpersonal view of testimony. According to that view, when a speaker *tells* a hearer that *p*, the speaker has invited the hearer to trust him or her (122). The hearer "is not being offered evidence, and accepting [the invitation] is not taking the testimony as evidence" (129). The trust the hearer has for the speaker is "irreducibly first personal" and is grounded in the hearer's relationship with the speaker (130).

The relationships that permit trust between speaker and hearer also provide the basis for communities of various sizes. The authority that other individuals have for me can be generalized to the broader community. There are two justifications for communal authority: either I conscientiously judge that I am more likely to get a true belief if I accept what the community believes or I conscientiously judge that I am more likely to get a belief that will survive conscientious self-reflection if I believe what the community believes than if I try to figure out what to believe on my own (155). Because I belong to the community and accept it as "an extended self," its authority is not an alien imposition on me (155). It shapes my thinking as I engage in critical self-reflection; for this reason, "the justification of authority in some cases can be partially circular" (157). This is not problematic, Zagzebski says, because "the authority has helped me attain a higher level of integrity of the self" (157).

Given this account of epistemic authority, it is easy to see how religious belief can be rational. If you belong to a religious community, its religious beliefs will have authority for you. When one of those beliefs is challenged, if it is "firmly rooted in a network of other beliefs and has already survived considerable self-reflection, it can be reasonable to treat putative evidence against it as either false or misleading" (214). This is so even when there is "a substantial amount of putative evidence against it" (215). Zagzebski cites as an example Alvin Plantinga's (1991) argument "that it is possible for a person rationally to be a young earth creationist in full knowledge of the evidence against it" (215n12). But here one might well think, as Hume (1991, sec. 7) says, that we "are got into fairy land, long ere we have reached the last steps of our theory."

The problem is that, of the two justifications for authority Zagzebski has offered—I conscientiously judge that I am more likely to get a true belief or I conscientiously judge that I am more likely to get a belief that survives critical self-reflection—it is the latter that serves in the justification of religious authority and in the resolution of intractable disagreements. The purpose of critical self-reflection is to resolve dissonance, as may arise when one is presented with counterevidence to a belief or when one has conflicting desires. But notice, in the case of conflicting desires, that the dissonance may be resolved by abandoning either of the desires: you can give up the desire to stay on your diet, or you can suppress the desire for chocolate—in either case, the dissonance is gone. This symmetry holds just as well in the case of beliefs: you can drop the belief, or you can ignore the counterevidence. In either case, the discomfort of dissonance is gone. But it is absurd to think that this way of proceeding can result in beliefs that are *epistemically* rational. The brainwashed members of a cult may have selves that are extremely harmonious, living in communities where neither dissent nor dissonance is tolerated, but they are living lives that are paradigmatically irrational.

A broader problem has to do with the preemptive reasons that epistemic authorities generate. Reasons of this sort, if there were any, would be very difficult to integrate into one's belief system. Suppose that an authority on the weather tells you that it will not rain all week; nevertheless, when you look out of your window, you can see rain falling. How can this dissonance be resolved? On the evidence picture, which Zagzebski rejects, the authority's testimony is evidence to be weighed against (and is presumably outweighed by) your perceptual evidence. But on the view Zagzebski favors, the authority "stands in" for you intellectually; his or her testimony preempts your evidence. This would obviously be the wrong result.

To be clear, Zagzebski does allow us to stop viewing particular others as authorities. The difficulty, though, is in understanding how this can happen.

The evidence that would be your basis for rejecting the authority appears to be *incommensurable* with the reason you derive from the authority.[1] On what basis, then, could you determine that the perceptual evidence is more trustworthy than the authority's reason? The most natural answer is that it is because your perceptual evidence is stronger than your evidence that the authority is a reliable testifier. But to say this is to retreat from the interpersonal view of testimony to the evidence view; it is to reject the idea that authority can be a distinctive source of epistemic reasons.

Zagzebski's book is much richer in conception and in detail than the brief summary of it given above. Her concern for the role of emotions in belief formation, evident in her work on the intellectual virtues, also finds expression here, and she examines a broader range of epistemic phenomena—including trust, autonomy, and the role of communities—than is usual in epistemology. The book comprises a complete epistemic worldview—one that will surely be very influential, both in epistemology and in philosophy of religion. Despite the misgivings raised above, I think it is an admirable and extremely valuable contribution to the literature.

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[1] For this criticism, see Lackey 2008, chap. 8.

Jesse Prinz, *The Conscious Brain: How Attention Engenders Experience.*
New York: Oxford University Press, 2012. xiii + 397 pp.

In his superb book *The Conscious Brain,* Prinz defends a theory of consciousness: a theory that (among other things) tells us what kind of thing consciousness is, what distinguishes different kinds of conscious states, and how consciousness is implemented in the human brain. Prinz's view is that conscious experiences are attended intermediate-level perceptual representations. Intermediate-level representations are, roughly, the kinds of representations that Marr (1982) had in mind when he discussed the "2 ½ D sketch": a 3D world is represented, but not in a way that abstracts from the viewpoint of the observer. For Prinz, such a representation is attended just if the information it represents is accessible to one of the set of postperceptual systems known as "working memory." The relevant representations are "vectorwaves" (coordinated patterns of neural firing that code information about features of the environment), and their attentive availability is implemented by synchronized firing in the gamma range across the relevant neural populations. This gives us an "unpacked" version of the view that provides an unusually vivid sense of how consciousness might actually be implemented in our neuronal wetware. The resulting discussion, drawing on a vast range of empirical data and astute philosophical reasoning, is absolutely essential reading for anyone interested in the topic. The comprehensiveness of topics discussed and the lucid and simple prose also make the book of great pedagogical value.

Prinz's view contrasts with views on which different *kinds* or different *degrees* of accessibility to postperceptual cognition are required for information to be conscious, or accessibility is not required at all. For example, Block's (2007) view is that consciousness doesn't constitutively involve postperceptual cognitive mechanisms at all; he thereby allows for conscious experiences that are more inaccessible to these systems than Prinz's view allows for. On the other extreme, some views require that conscious perceptual information be actually encoded in these postperceptual systems, not merely accessible to them; for example, this is what we get on Dehaene and Naccache's (2001) neuronal global workspace model. The view also differs from views on which conscious experience doesn't exclusively involve intermediate-level content. For example, it might also involve more high-level representations, such as those involved in object categorization, as on Siegel's (2010) view.

Both aspects of the view raise epistemic and explanatory problems. What is the right methodology for figuring out when consciousness arises in the brain? Prinz's methodology is to figure out empirically the conditions under which states in the brain fit certain pretheoretical functional criteria we associ-

Thanks to Ned Block for helpful comments on an earlier draft.

ate with consciousness (in particular, forms of access-consciousness like a subject's ability to report information), and then conclude that these are the conditions under which consciousness exists. This makes it unsurprising that for him, consciousness is a kind of accessibility. One problem with this is that arguably there is no a priori guarantee that there aren't conscious states that don't fit the pretheoretical criteria (as Chalmers [1998] pointed out). How then do we know that there aren't conscious states that are not accessible to postperceptual cognition? A second problem is that even if the method correctly tells us when consciousness arises in the human brain, how do we know it isn't merely uncovering contingent features of how consciousness is *implemented* in us, rather than *essential* features? We want to know what consciousness *is*, not merely when it contingently occurs in us. A third kind of worry is explanatory. It is tempting to ask: *why* does consciousness have the features Prinz says it does? This becomes especially salient when we see that some of these features seem to be quite arbitrary, as I will explain.

Why does Prinz reject inaccessible, unreportable experiences? Ultimately, his reasoning turns on something like the following principle: unless we have positive evidence that such inaccessible states exist, then we are justified in believing that they *don't* exist (105, 107–9). But although this is not implausible, it is unclear what exactly justifies this principle. It might seem like a straightforward application of theoretical parsimony. But on reflection, given the existence of some representation in the brain, saying that it's conscious isn't any less parsimonious that saying it isn't. Inaccessible experiences aren't like invisible ghosts; perhaps a better analogy is discovering a new kind of particle and not knowing whether it is positively charged. Second, the method of pretheoretically associating conscious experience with forms of access-consciousness will likely prevent us from finding evidence for inaccessible states, so an absence of evidence is hardly evidence of absence in this case. Finally, Prinz's view of phenomenal concepts seems to commit him to agreeing with Chalmers that there is no conceptual or semantic guarantee that there aren't conscious states that fail to fit our pretheoretical functional criteria. So we are left wondering what justifies Prinz's basic epistemic principle.

The kind of accessibility that Prinz associates with consciousness also invites explanatory puzzlement. For example, you might think that being accessible to working memory would require that working memory can actually use the relevant intermediate-level information. But, on the contrary, it turns out that working memory doesn't use intermediate level information *at all*, but rather more abstract high-level information. The intermediate-level information is "accessible" only in the sense that it is the input into a process of high-level categorization that leads to high-level representation in working memory (101–2). This makes one think—what's so special about this rather awkwardly indirect form of accessibility? Why is *that* what constitutes consciousness?

Prinz could reply that consciousness occurs at the intermediate level and that this is the kind of accessibility that applies to these representations. But, of course, that invites the reply: what's so special about the intermediate level? Although Prinz argues that, *in fact*, all conscious representations are intermediate level, he doesn't explain *why* consciousness is constrained in this way. One thing that makes this especially puzzling is that he rejects representationalism, holding that qualitative character is determined by intrinsic features of the representational vehicle (a "vectorwave"), not its content (11–21, 126–33). But if informational content is irrelevant to qualitative character, why does it matter for consciousness?

One can ask the same kind of explanatory questions about other features of Prinz's view. How might he respond? Rather than tackling them head on, he might reject such questions as illegitimate or based on false presuppositions. For example, he might say that theoretical identities are not the kind of thing that can be explained—more on this below. Another option is to say that he is only telling us how consciousness is contingently *implemented*, not what it is essentially. For example, maybe there's nothing about consciousness that requires it to involve only intermediate-level representations; it's just a contingent fact that *in us*, these are the only representations that meet the relevant conditions (Mole [2013] suggests this retreat in his review). Retreating to a view of implementation rather than essence gives the account less explanatory depth, but at least it might avoid some of these hard explanatory questions. It raises the question: how do we draw the line between essence and implementation?

Prinz (2003) has previously taken a mysterian approach to this issue, holding that this dividing line is unknowable; one could also be deflationary about it (see below). But in *The Conscious Brain*, he thinks we can do better. The relevant level is "a level of analysis that can account for every aspect of conscious experience, in the sense that every conscious difference can be mapped onto a difference at that level" (289). The idea is supposed to be that absent positive evidence that a physical feature is essential, we should assume it isn't (echoing the methodology mentioned above); in effect, we should take consciousness to be as high level as we can. This suggests that Prinz might indeed err on the side of saying that the features of his account concern only how consciousness is contingently implemented.

However, we can question Prinz's principle here. We don't seem to adopt it in other cases; for example, taking water to be H_20 probably wasn't taking the most high-level view available. Furthermore, what counts as a "conscious difference"? If we're talking about differences in what someone reports, or discrimination behavior more generally (and this appears to be what he has in mind), then what differences are relevant? Even very fine-grained features of neural states carry information about the environment and can have a small statistical impact on discriminatory abilities. So if we want to explain *every* aspect of a subject's discriminatory capacities, we'll need to look at the brain at a very fine-grained level. But if only less fine-grained statistical features of their discriminatory behavior are relevant, then what's the relevant level of grain? On the other hand, if we're talking *directly* about experiential differences, rather than discrimination behavior, then the problem is that we don't have any direct way of measuring these differences, so we are pushed back toward his earlier mysterian view.

Prinz could also respond to the question "why does consciousness have feature X?" by saying that theoretical identities *can't* be explained. Consciousness just is what it is. In my view, this doesn't satisfactorily explain away the sense of unacceptable arbitrariness in aspects of the view (for example, why only intermediate-level representations?). What else might he say? Prinz expresses an interestingly deflationary attitude toward theoretical identification in other cases. He writes, "Every theoretical identification involves some degree of arbitrary decision" and that "the vast majority of theoretical identities by scientists are actually pragmatically and politically brokered decisions rather than discoveries" (299). If he took this view with consciousness, he could say that details of his view appear arbitrary and explanation demanding because they really *are* arbitrary, in the way that *all* theoretical identities are.

But his view is that the case of consciousness is quite different from this: "We assume that scientists are not making an arbitrary choice when they decide between different neural correlates" (300). I suspect that Prinz should take more seriously a similarly deflationary attitude toward questions about the essence of consciousness. Imagine a Martian scientist explaining our behavior by reverse engineering the human brain. It's not as if one of the consciousness-candidates under discussion here is going to stand out to them as having a special glow. Relatedly, if they are trying to interpret our term "consciousness," it might seem rather arbitrary and uninteresting what *exactly* the interpretation of the term ought to be. If we are materialists and we think that all that really exists are the kinds of facts that this Martian scientist would know about, shouldn't we have a fairly deflationary attitude toward finding the nature of consciousness that mirrors how things would look to the Martian scientist?

Compare the case of life: we might be worried about how life could possibly be a physical phenomenon. But once we have overcome these worries, it seems to be a mistake to worry about exactly what the necessary and sufficient conditions for it are. We learn what life is by learning about the physical processes that underwrite its specific manifestations around us. Who cares how much of this is strictly necessary for "life"? (Notice that even if we are deflationary about what exactly life is, that doesn't require us to think that it is unimportant: for example, we could still think that it is astonishing that the universe contains such a thing.) Similarly, we learn a lot about what consciousness is by learning about the specific processes that underwrite it in the brain, but perhaps there's no deep fact about which parts of this processing are really required (this is not to say that it's *completely* indeterminate: some views might be determinately wrong, just as some views of what life is are determinately wrong). Adopting such a perspective might enable Prinz to offer deflationary responses to both the epistemic and explanatory challenges I raised earlier.

Against this, it's true that Prinz argues that the analogy between reducing consciousness and other theoretical reductions breaks down because of the unique kind of epistemic access we have to consciousness. The issues here are complex, but I suspect it is a mistake to think that this *epistemic* asymmetry justifies a more inflationary *metaphysical* view. The real significance of the epistemic disanalogy comes in explaining, or explaining away, the philosophical puzzlement surrounding consciousness: how *could* it be something physical? Even if we are deflationary about the metaphysics of consciousness, there is still the highly significant philosophical task of ridding ourselves of this explanatory puzzlement.

Despite these misgivings, I still think this is a great book. If you haven't read it yet, you should. If you have already read it, read it again!

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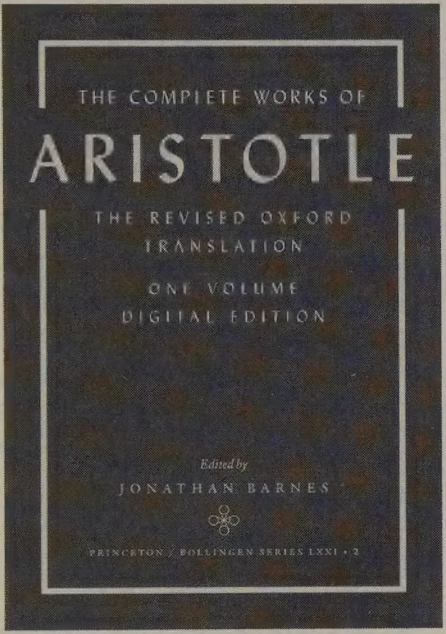

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